PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCIX.

PART II.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
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CONTENTS.

X. *On Platina and native Palladium from Brasil.* By William Hyde Wollaston, *M. D. Sec. R. S.* p. 189

XI. *On a native Arseniate of Lead.* By the *Rev.* William Gregor. *Communicated by* Charles Hatchett, *Esq. F. R. S.* p. 193

XII. *An anatomical Account of the Squalus maximus (of* Linnæus), *which in the Structure of its Stomach forms an intermediate Link in the Gradation of Animals between the Whale Tribe and cartilaginous Fishes.* By Everard Home, *Esq. F. R. S.* p. 212

XIII. *On an Improvement in the Manner of dividing astronomical Instruments.* By Henry Cavendish, *Esq. F. R. S.* p. 221

XIV. *On a Method of examining the Divisions of astronomical Instruments.* By the *Rev.* William Lax, *A. M. F. R. S.* Lowndes's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. Astronomer Royal.* p. 232

XV. *On the Identity of Columbium and Tantalum.* By William Hyde Wollaston, *M. D. Sec. R. S.* p. 246

XVI. *Description of a reflective Goniometer.* By William Hyde Wollaston, *M. D. Sec. R. S.* p. 253

XVII. *Continuation of Experiments for investigating the Cause of coloured concentric Rings, and other Appearances of a similar Nature.* By William Herschel, *LL. D. F. R. S.* p. 259

XVIII. *An Account of a Calculus from the Human Bladder of uncommon Magnitude.* By Sir James Earle, *F. R. S.* p. 303

XIX. *On expectorated Matter.* By George Pearson, *M. D. F. R. S.* p. 313

XX. *On the Attractions of homogeneous Ellipsoids.* By James

Ivory, *A. M. Communicated by* Henry Brougham, *Esq. F. R. S.* p. 343

XXI. *Observations on Albumen, and some other Animal Fluids; with Remarks on their Analysis by electro-chemical Decomposition. By Mr.* William Brande, *F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.* p. 373

XXII. *Hints on the Subject of animal Secretions. By* Everard Home, *Esq. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.* p. 385

XXIII. *On the comparative Influence of Male and Female Parents on their Offspring. By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.* p. 392

XXIV. *On the Effect of westerly Winds in raising the Level of the British Channel. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S. By* James Rennell, *Esq. F. R. S.* p. 400

XXV. *On Respiration. By* William Allen, *Esq. F. R. S. and* William Hasledine Pepys, *Esq. F. R. S.* p. 404

XXVI. *Experiments on Ammonia, and an Account of a new Method of Analyzing it, by Combustion with Oxygen and other Gases; in a Letter to* Humphry Davy, *Esq. Sec. R. S. &c. from* William Henry, *M. D., F. R. S. V. P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester.* p. 430

XXVII. *New analytical Researches on the Nature of certain Bodies, being an Appendix to the Bakerian Lecture for* 1808. *By* Humphry Davy, *Esq. Sec. R. S. Prof. Chem. R. I.* p. 450

Presents received by the Royal Society, *from November* 1808 *to July* 1809. p. 471

Index.

PHILOSOPHICAL TRANSACTIONS.

X. *On Platina and native Palladium from Brasil.* By William Hyde Wollaston, *M. D. Sec. R. S.*

Read March 23, 1809.

Although platina has now been known to mineralogists for more than sixty years, yet it had not been discovered in any other places than Choco and Santa Fé, whence it was originally brought, until about two years since M. Vauquelin discovered it in some gray silver ores from Guadalcanal in Estremadura. In analysing these ores, he found some fragments that contained as much as one tenth of their weight of platina, but he did not find it accompanied by any of the new metals that have lately been discovered in the Peruvian ore of platina.

The specimen which I am now about to describe, is derived from a third source, and it is rendered the more interesting by having grains of native palladium mixed with it. This new mineral has lately been received from the gold mines in Brasil, by H. E. Chev. de Souza Coutinho, ambassador from the court of Portugal, resident in this country, and I am in

hopes that some account of it may be acceptable to the Royal Society, although the analysis must necessarily be very imperfect, from the small quantity to which my experiments have unavoidably been confined.

The general aspect of this specimen is so different from the common ore of platina, that I could form no conjecture of what ingredients it might be found to consist. Its appearance was such indeed, as at first sight to induce a suspicion of its not being in a natural state, for it had very much the spongy form which is given to platina from imperfect attempts to render it malleable by means of arsenic.

One circumstance, however, occasions a presumption that no art has been employed in giving the grains their present appearance; as upon close inspection many small particles of gold are discernible, but there is none of the magnetic iron sand with which the Peruvian ore abounds, nor any of the small hyacinths, which I have formerly noticed as accompanying that mineral.*

It is very well known, that the common ore of platina in general consists of flattened grains, that appear so much worn at their surface, as to be in a considerable degree polished, and the roughness observable in some of the larger grains arises from concave indentations of a reddish brown or black colour. The Brasilian platina, on the contrary, has no polish, and does not appear worn; but most of the grains seem to be small fragments of a spongy substance, and even those which are yet entire and rounded on all sides, present a sort of roughness totally different from that of the former, as their surface consists of small spherical protuberances closely coherent to each

* Phil. Trans. for 1805, p. 318.

other, with the interstices extremely clean, and free from any degree of tarnish.

The first portion that I employed for solution was taken without any selection, and being digested with a small quantity of nitro-muriatic acid, two of the grains were acted on much more rapidly than is usual with platina, and seemed to give a redder colour than that metal alone. These grains were consequently taken out, washed, and reserved for separate examination, and the solution was allowed to proceed till the rest were entirely dissolved. By the addition of muriate of ammonia an abundant precipitate was formed of a bright yellow colour. This precipitate was evidently platina, and its colour satisfied me that the grains had not been brought into their present state from Peruvian platina by means of arsenic; for where arsenic has been employed, I have observed that the iridium contained in that ore is rendered more soluble than before, and thence communicates its red colour to the precipitate.

From the grains thus examined, there appeared not to be any iridium dissolved, nor any black powder containing iridium undissolved.

I next endeavoured, by prussiate of mercury, to ascertain the presence of palladium, but though a precipitate which occurred indicated a certain quantity, it remained doubtful whether it was derived from the grains of platina themselves, or from the two small fragments that had been in part dissolved before they were separated from the rest.

By addition of ammonia to the solution, no iron was precipitated; and when the solution was afterwards allowed slowly to evaporate, I could discern no crystals or colour that I could

ascribe to the presence of rhodium. In short, it seemed that these grains are really native platina nearly pure.

In order to discover whether the grains themselves contained any portion of gold, I selected three of the largest weighing together eight grains and a half; and after a solution and precipitation, as before, by muriate of ammonia, I added a solution of green sulphate of iron, and obtained a precipitate of gold. It was, however, far too small in quantity to be estimated with correctness, but certainly did not exceed the $\frac{1}{200}$ of a grain. This, it is to be observed, is another circumstance in which the present mineral differs from the Peruvian ore of platina, which I believe never contains (in the ore itself) the smallest quantity of gold.

In this experiment also, I tried to detect the existence of palladium in the solution, and by prussiate of mercury again ascertained its presence; but it was in too small quantity for estimating the proportion it bore to the whole mass.

It may deserve to be remarked, that though neither the Peruvian nor Brasilian grains of platina contain any silver, yet the gold which accompanies them is in each instance so much alloyed with silver, that from about thirty small scales of gold picked from Peruvian platina, weighing two grains, I obtained as much as four tenths of a grain of silver, or one fifth part of their weight.

Native Palladium.

The two fragments, that had been separated from the first solution, next claimed my attention, and evidently deserved a careful examination. They were each placed in a drop of nitric acid, and each communicated a deep red colour, which,

by the tests of prussiate of mercury and green sulphate of iron, I was satisfied arose from palladium. The smaller fragment was then divided, and one portion allowed to remain in the acid till it seemed completely dissolved, and the other examined by the blow-pipe. The utmost heat that could be given, appeared to have no effect; but when a small piece of sulphur was applied to it, it fused instantly; by continuance of the heat, it parted with the sulphur, and became completely malleable. In short, it perfectly resembled palladium; and as it retained its brilliancy in cooling, I judged it to be nearly pure.

But as the surfaces which had been acted upon by nitric acid had a degree of blackness, that might be owing to some insoluble impurity, I have since that time dissolved the larger fragment for the sake of discovering the cause of this appearance. Hot nitric acid dissolved by far the greatest part; but there remained a black powder on which a fresh addition of this acid alone had no further effect. But when a drop or two of muriatic acid was added, the whole was very soon dissolved. By the addition of muriate of ammonia, it became evident from the precipitate that the residuum was principally platina. But this precipitate, instead of being yellow, had the deep red colour, which is usually occasioned by the presence of iridium. The platina reduced from this precipitate was also too black for pure platina, and when it was again dissolved, the solution was of a deep red, and the precipitate by muriate of ammonia red, as before; so that although the grains of Brasilian platina appear to be free from iridium, as well as from many other impurities that form part of the Peruvian ore, yet the grains of native palladium that accompany them, afford a trace

of this ingredient, and occasion a presumption that osmium and rhodium may hereafter appear, when we can obtain this mineral in larger quantity.

Since the whole weight of metal employed in the last experiment did not exceed $1\frac{5}{10}$ grain, it is in vain to attempt to estimate the proportion of the ingredients, but if I am near the truth, in considering the quantity of the red precipitate as about one fifth of a grain, of which less than half is platina, those who are best acquainted with the intense colouring power of iridium may endeavour to form a conception of the extremely small quantity that can be present.

As soon as I had ascertained the existence of native palladium, I endeavoured, by examination of its external characters, to distinguish its appearance from that of the surrounding substances, and I found it by no means difficult, although no difference of colour could be discerned. Having remarked that the larger fragment appeared rather fibrous, and that the fibres were in some degree divergent from one extremity, I examined the remainder of the small specimen which had originally been given to me, and by this peculiarity of structure I soon detected a third fragment, which upon trial proved to be the same substance. By favour of the Chev. DE SOUZA I was also permitted, with this view, to examine the specimen which remained in his possession, and had soon the satisfaction of discovering two more fragments of the same mineral, and as I was in no one instance deceived in my choice, by attending to the radiating fibres, I am in hopes that this external character will enable persons to distinguish that metal, in situations where they have not an opportunity of deciding by chemical experiment.

XI. *On a native Arseniate of Lead.* By the *Rev.* William Gregor. *Communicated by* Charles Hatchett, *Esq. F. R. S.*

Read April 13, 1809.

I.

THAT the oxide of lead and the arsenic acid, might be found in the state of natural combination, is a supposition highly probable, from the strong affinity which subsists between these two substances. But the existence of such a compound has not, as I conceive, hitherto been established by such proofs, as entitle it to be ranked amongst the *decided cases* of mineralogical science. I trust, therefore, that the observations, which I have the honour of submitting to the Society, on a new* ore of lead lately discovered in the county of Cornwall, so justly celebrated as well for the variety as for the richness of its mineral productions, will not be deemed superfluous.

This mineral was raised in the mine called Huel-Unity, a very rich copper mine, in the parish of Gwennap. According to the information with which I have been favoured by Mr. WILLIAM DAVEY, a very intelligent and experienced miner in that district, it was found in a lode south of Huel-Unity principal lode, at the depth of fifty fathoms below the surface,

* It is new at least to the miners in Cornwall; nor was there, previously to this discovery, any ore resembling it to be found in that splendid collection of minerals, which my valuable friend PHILIP RASHLEIGH, Esq. has so liberally formed, and so liberally employed in the promotion of Science.

which lode underlay about two feet in the fathom south: at the depth abovementioned; this lode fell in, or formed a junction with another small lode or vein to the south, and when the junction took place, this lead ore was found. The veins of it are, in general, from six to ten inches wide, and they diverge on going west. Some particles of this lead ore have been found in the southern part, after the separation of the lodes; but the northern lode does not contain any, until the junction takes place. This ore is intermixed with some native copper, very rich gray copper, and black copper ore, and some is mixed with quartz. The walls of both veins are killas.

II. *Description.*

This mineral is regularly crystallized. The form of its most perfect crystals is an hexaedral prism; they are of different sizes, from one tenth of an inch in diameter, to the size of a hair. The longest which I have seen, do not exceed three tenths of an inch in length: these terminate in a plane, at right angles, with the axis of the prism; but the crystals of a smaller size are frequently drawn out into a very taper acumination, which appears to be a six-sided pyramid. A number of smaller crystals are often closely packed together in bundles, which are bent in different directions, and terminate in a point. The larger crystals, either stand alone, or adhere, on their lateral planes, to the gangue, or are confusedly matted together in a mass.

Some of them are hollow, as if an internal nucleus had been destroyed: and sometimes this internal nucleus overtops the external laminæ. The gangue is a white quartz, which fre-

quently exhibits on its surface the appearance of a partial decomposition.

The red octaedral copper ore, and the copper into which that ore passes, are often intermingled with the crystals of this lead ore and inbedded in them.

The colour of these crystals consists of a variety of tints of yellow. Some are of a beautiful wine yellow resembling the Brazilian topaz: this, in the greater number of specimens, passes into a delicate Isabella-colour: whilst, in other cases, we have the honey-yellow mingled with brown hues of different intensities: so that we meet with crystals resembling dark brown sugar-candy, or common resin.

Some of the crystals are beautifully transparent, whilst others possess this quality in part only, at their extremities, or in inferior degrees throughout their whole lengths.

The external lustre in some specimens, is vitreous; in others, resinous: but in some instances their surface is partially covered by tender and delicate filaments of a silky lustre. These filaments are sometimes found in a separate state loosely adhering to quartz; and they form a variety of this fossil.

The crystals vary as to hardness. The angular fragments of the most transparent are sufficiently hard to scratch glass.

This mineral is easily reduced to powder, which has the appearance of pounded resin; it contracts a yellower tint by long exposure to the air.

The specific gravity of the purest crystals, taken at the temp. 50° FAHRENHEIT, was 6.41.

III.

A fragment of crystal exposed to the flame of the blow-pipe in a gold spoon, melted into a brownish yellow mass, which on cooling did not assume any angular figure. It remained in a state of ignition apparently unaltered; but when a piece of it was exposed to the flame on charcoal, a rapid decomposition took place, arsenical vapours were extricated, and globules of a metal, possessing the common properties of lead, were left behind.

This mineral, in a state of fine powder, is soluble in nitric acid, even without the aid of heat. Care, however, must be taken, that it does not concrete into lumps. The vessel therefore which contains it must be frequently shaken, and the nitrat of lead produced must be, from time to time, dissolved in water, and poured off from the residuum. The process of solution is, however, accelerated by a digesting heat. Some silica remains, which, as the quantity of it is variable according to circumstances, appears not to be an essential ingredient of this fossil.

The nitric solution is colourless; its transparency is not disturbed by nitrat of barytes. Nitrat of silver renders it turbid, and a small quantity of white curdly matter is deposited. Sulphuric acid and the liquid sulphats, produce copious precipitates of a white heavy matter. If the fluid be poured off from this subsided matter, and it be freed from the superfluous sulphuric acid, by the means of nitrat of barytes, it will yield, on the affusion of liquid nitrat of lead, an abundant white precipitate, which urged by the flame of the blow-pipe on a support of charcoal, resolves itself into reduced lead and arsenical vapours.

These preliminary experiments led me to the probable conclusion, that this fossil chiefly consisted of oxide of lead, arsenic acid, and a small quantity of the muriatic acid.

IV. *Analysis.*

A.

1. Fifty grains, carefully selected from crystals of a pale Isabella-colour, were reduced to a fine powder and exposed to a low red heat for about an hour. Their weight was diminished by 0.15 of a grain.

2. The yellowish powder was now transferred to a vessel of pure silver, and mixed with a lixivium containing fifty grains of potash, prepared by the means of alcohol; a quantity, which I had previously ascertained to be sufficient to effect a complete decomposition of this mineral. The ley was gradually evaporated to dryness in a sand-bath. The soluble part was extracted by distilled water, and poured off from a yellowish white matter, which was sufficiently edulcorated (*a*).

3. Liquid nitrat of ammonia was now dropped into the alkaline fluid, as long as it produced any cloudiness: the clear fluid was now decanted from a small quantity of white matter, which had subsided, and rendered acid by nitric acid; ammonia, added to excess, produced a slight turbidness. These precipitates, after sufficient edulcoration, were added to the yellowish white residuum (*a*).

4. The liquid was now rendered slightly acid by nitric acid, and a solution of nitrat* of lead in distilled water was dropped

* If the colourless liquid *oxynitrat* of lead be dropped into a dilute solution of arsenic acid, or of arseniat of potash acidulated by nitric acid, no immediate precipita-

into it, as long as it separated any precipitate. The clear fluid was poured off, and evaporated nearly to dryness, and a small quantity of white matter, thus obtained, was added to the former precipitate, which dried, and exposed to a low red heat weighed, whilst still warm, 40,8, which, according to the proportion of 33 : 100, established by Mr. CHENEVIX, implies 13,46 of arsenic acid.

5. The superfluous lead was now separated from the fluid by sulphat of soda, and filtered off. Ammonia precipitated a minute portion of flaky matter; it weighed, after ignition, 0.2 of a grain; it consisted of silica and oxide of lead, and must be attributed to the nitrat of lead employed.

B.

1. The yellowish white residuum (*a*) (A, § 2.) was dissolved without effervescence in nitric acid, except a minute portion of silica, which, after ignition, = 0,28. A white heavy matter was thrown down from this solution, by liquid sulphat of soda. The clear decanted fluid was evaporated to a small volume, and sulphat of soda produced a further separation of white matter; It was sulphat of lead, which after exposure to a low red heat, and weighed, whilst warm, = 47.5, which, upon the supposition that one hundred parts of sulphat of lead contain 69.74 of lead + 3.48 of oxygen, is equivalent to 34.77 of oxide of lead.

2. The fluid, now freed from lead, deposited, on the affu-

tion of an arseniat of lead is produced; but crystalline grains are, after a time, gradually deposited at the bottom of the vessel. But liquid *nitrat* of lead causes an immediate and abundant precipitate from these same dilute solutions. These two combinations therefore must be different.

sion of ammonia, a greenish matter, which, after ignition, became red, and = 0.033 of a grain. It was oxide of iron.

C.

1. One hundred grains of larger crystals, some of which were hollow, and the surfaces of which were slightly and partially covered with silky filaments, treated in the same way yielded 95.283 of sulphat of lead equivalent to 69.76 of oxide, and 80 of arseniat of lead, which indicates 26,40 of arsenic acid. The oxide of iron, in this case, amounted to only .05 of a grain, and the residuary silica was in too small a quantity to be weighed.

2. I have endeavoured to decompose this fossil by boiling it to dryness in a solution of four times its weight of the purest subcarbonat of potash, and exposing the dry mass, for a very short time, to a low red heat; but I found, that only a part of the arsenic acid had united to the alkali; the larger portion of it was detected in the nitric solution of the residuum; but the relative proportions of the oxide and the acid, were found to correspond almost exactly with the foregoing statement of them.

3. I found also, that carbonat of ammonia precipitated this mineral, *in an unaltered state*, from its solution in nitric acid: as no arsenic acid had united with the precipitant. The solution of the nitrat of ammonia was evaporated to dryness, and exposed to a red heat in a platina crucible; but nothing was left, except a slight trace of oxide of lead. We may infer from hence, the absence of both the fixed alkalies.

4. I found in one specimen only of this fossil any notable difference in the relative proportions of the oxide of lead and

of the acid, to which it is united. It consisted of crystals confusedly matted together in a more compact mass, than this fossil generally assumes. One hundred grains were dissolved in nitric acid; the marine acid was separated by nitrat of silver, and any redundant silver by muriat of ammonia. The lead was separated by sulphuric acid, and the superfluous portion of that acid by nitrat of barytes, and the arsenic acid was combined with the oxide of lead by the affusion of nitrat of lead. The muriat of silver = 9,8; the sulphat of lead = 97.6, and the arseniat of lead = 72, equivalent to 1.63 of muriatic acid, 71,46 of oxide of lead, and 23,88 of arsenic acid, respectively. The quartz = 0.35, and the oxide of iron .0.2, nearly,

Another portion taken from the same specimen treated with an alkali, gave very nearly a similar result.

D.

It will now be necessary for me to speak concerning an ingredient of this fossil, which I may have seemed to overlook. I mean the muriatic acid: I have found some difficulty in ascertaining the proportion which it bears to the other constituent parts, and from a cause, which I did not suspect. I considered that the only sure mode of determining this point, was to have recourse to nitrat of silver, which might effect a direct separation of the marine acid from the nitric solution of this fossil. But I found, in many experiments upon given quantities of this mineral, that the results, which I derived from this most valuable chemical test, were variable and uncertain.

At last, I was enabled to trace the error and uncertainty up to two sources. In the first place, I found that the muriat of

silver was more abundant in the cases, where I employed a vessel with a long neck for the solution, and *did not expose it to heat*.

I concluded therefore, that when the process was conducted under different circumstances, the predominating *mass* of nitric acid produced its effect, and volatilized a portion of the muriatic.

Another source of error I found in the following anomalous circumstance, *viz.* a simultaneous precipitation of a portion of arseniat of lead takes place with that of the muriat of silver. Whatever combination this may be, it is a weak one, and may be severed by nitric acid, which dissolves the arseniat and leaves the muriat; or by ammonia, which takes up the muriat, to the exclusion of the arseniat.

The conclusion, to which many experiments have led me, is this, that the muriat of silver produced in the nitric solution of one hundred grains of arseniat of lead by nitrat of silver, amounts to about 9.5.

E.

In order to prove that the acid, which is combined with the oxide of lead in this mineral, *is the arsenic acid*, and that it is not combined with phosphoric, I decomposed some of its acid, which had been combined with lead in the foregoing experiments, by means of sulphuric acid, and filtered off the sulphat of lead. The fluid which passed through the filter was evaporated nearly to dryness, and it assumed the appearance of crystalline grains. Some of it was exposed to the flame of the blow-pipe in a gold spoon; at first it became like a white dry powder, which melted before an increased heat: placed

on *charcoal* and ignited, it was totally dissipated in arsenical fumes.

Some of it was dissolved in water, and dropped into liquid sulphat of titanium, a white precipitate was produced: combined with soda, it precipitated silver from the nitrat of silver, of a brick colour. It precipitated mercury from its nitrat, of a yellowish colour, which afterwards became reddish. This precipitate exposed to the flame of the blow-pipe on charcoal, exhibited the same phenomena as arseniat of mercury.

I precipitated magnesia from its muriat, and redissolved it by carbonat of ammonia, perfectly saturated with carbonic acid. I divided this liquid into two portions, and dropped into *both* a solution of the combination of the acid of this mineral and soda. No precipitate was produced. I dropped into *one* of the vessels some liquid phosphat of soda, and a separation of saline matter was instantly produced. I soon, however, found, that this mode of distinguishing the phosphoric from the arsenic acid could not be depended upon. For in the other vessel, in which no phosphat of soda had been dropped, in a short time, saline tufts made their appearance, and an abundant deposition of saline matter was formed. I found also, that if the solution had been more concentrated, the precipitation would have immediately taken place.

On making a comparative experiment with arsenic acid, I found that it forms a triple salt with ammonia and magnesia, analogous to the phosphoric salt described by Dr. WOLLASTON. The figure of the arsenical salt, as far as I could determine it from a confused crystallization, is a triedral prism.

We are therefore, I think, authorized from the experiments herein detailed, to conclude, that the fossil which is the subject

of this paper, is an arseniat of lead, and that if we state that the relative proportion of the constituent parts of it are in one hundred, as follows, we shall not be far from the truth:

Oxide of lead -	69.76
Arsenic acid -	26,40
Muriatic acid - -	1.58

The silica and the oxide of iron, which account for a portion of the loss, and the alumina and copper which are sometimes found in an analysis of this fossil, I do not conceive to be essential to it.

The existence of a minute portion of muriatic acid as a constant ingredient of it, is a curious fact: and it is still more curious, when we consider it in connexion with the analogy, that in this particular it maintains with the natural phosphats of lead.

Creed, March 1, 1809.

XII. *An anatomical Account of the Squalus maximus (of* Linnæus), *which in the Structure of its Stomach forms an intermediate Link in the Gradation of Animals between the Whale Tribe and cartilaginous Fishes. By* Everard Home, *Esq. F. R. S.*

Read May 11, 1809.

THE fish from which the following account is taken, was entangled in the herring nets belonging to the fishermen of Hastings, off that coast, and about half-way across the Channel, on the night of the 19th of November, 1808. It was brought ashore at Hastings on the following day, and my late worthy friend, Lieut. Col. BOTHWELL, who was on the spot, purchased it on my account. On the 17th, Mr. CLIFT, the Conservator of the Hunterian Museum, at my desire, went to Hastings, and after making a drawing of the fish, examined its internal structure, and brought to London such parts as were most particularly deserving of notice.

The fish is a male, thirty feet six inches long from the anterior part of the head, to the longest extremity of the tail, and about nine feet from the extreme point of the dorsal fin to the middle line of the belly.

The skin is of a dirty blue, or light slate colour; as rough as a new file in the direction from the tail to the head, but having a sattiny feel in the opposite direction. On the belly the skin is white, thick, and very strong.

The mouth is about five feet from one angle to the other, There are six rows of teeth towards the middle of each side

of the jaw; but in the other parts they are less numerous. The teeth are small, round, conical, very pointed, and bent a little inwards.

The nostrils open on the edge of the upper lip. The eyes are very small, and the pupils perfectly round.

Half-way between the eye and the gills, on each side, is the orifice of a canal, which leads into the mouth.

The gills are five in number on each side.

The pectoral fins are situated a little behind the posterior gills.

The dorsal fin is situated nearly opposite to the middle space, between the pectoral and anal fins. The posterior dorsal fin is small, and situated half-way between the anal fins, and the setting on of the tail. The two anal fins are attached on their upper edge for about half their extent each to the lower side of a long projecting body peculiar to the male. All the fins have a thick round edge anteriorly, and become gradually thinner towards the posterior part, which is partially serrated.

The projecting bodies on the sides of the anus I shall call holders, as they are employed in grasping the female in the time of copulation. They are rounded on their lower surface: the skin covering them is uncommonly thin and smooth: on their upper surface, it has a gloss like that of silk, and there is a deep sulcus, in which is contained a strong, flat, sharp, bony process, five inches long, which moves on a joint, and the bone projects an inch and a half beyond the skin, like a spur.*

There is a deep sulcus at the setting on of the tail, as if a

* I shall give a particular description of these holders in a future paper on the generation of the dog-fish.

rope had been tied tight round that part, and on each side of the fish, there is a scabrous ridge extending from this sulcus as far forwards as the posterior dorsal fin. The tail may be said to begin from this deep transverse sulcus; it is vertical; the upper portion is longest and narrowest. The thin edge of the tail has a jagged appearance, as if formerly wounded by the bites of small fishes.

The proportions of these parts to each other will be seen in the annexed figure.

In this fish no part of the skeleton can be said to be perfectly formed bone, although the skull, which defends the brain, the upper and under jaws, and the vertebræ, contain bony matter; the vertebræ however in much the smallest proportion.

The skull has no cartilaginous attachment to the upper jaw, so that it is readily separated from it, while the jaws themselves are strongly connected by means of the joint between them. The skull, when the jaws are removed, is very small, being adapted to the size of the brain, which in this fish bears a very small proportion to that of the whale tribe.

The vertebræ next the head are rather smaller than those a little further on. The largest are seven inches in diameter, the cavities between them containing about three pints of fluid. A particular account of the intervertebral joint in this fish has been already laid before the Society.

On the posterior part of the vertebræ, there is a sulcus, in which a strong elastic ligament passes from the head almost to the tail. The canal for the spinal marrow is very small; and on the anterior part of the vertebræ, below the anus, there is a canal, in which is contained the aorta.

Each gill is supported on two cartilages, one long and flat connected with the spine, the other short; there is a joint between them which admits of some degree of motion. There are cartilages which answer the same purpose, as the sternum and scapulæ of quadrupeds. Those resembling the scapulæ are connected to the spine, and to them the cartilages of the pectoral fins are articulated.

The broad part of the pectoral fin is formed of a number of cartilaginous finger-like processes. The terminations of which are inclosed between two sets of ligamentous fibres, that form the thin margin of the fin.

The other fins and tail are formed in the same manner, and are also connected with the spine.

There is an imperfectly formed pelvis, connected to the spine by strong ligaments and muscles. The cartilages of the holders are united to the pelvis; they correspond in number and general appearance to a femur, tibia, and three toes. This part has been frequently mistaken by superficial observers for the leg and foot, to which it bears a very general resemblance.

The heart is not much larger than that of a bullock: the auricle is extremely thin in its coats. The valves at the origin of the pulmonary artery, are three in number; besides which there are three sets of valves in the course of the artery, at a short distance from each other. Each set is composed of three valves. They are weaker than those at the origin of the artery, and they are attached by tendinous chords to the inside of that vessel.

The tongue is flat, and can hardly be said to have any part of it loose and pendulous.

The œsophagus is about a foot in length: its internal surface is milk white and polished, having a number of conical papillæ projecting into the canal. These become gradually longer towards the stomach; they all point downwards, and the lower ones have a fringed appearance, which is shewn in the figure.

The stomach, from its weight, could not be removed till emptied of its contents, which consisted of several pails full of pebbles, a quantity of mucus, and a small portion of a substance, which proves to be the spawn of a univalve. The appearance of the internal structure of the stomach, and the dimensions of its different parts are shewn in the drawing. Besides the cardiac and pyloric portions, as in other sharks, there is a globular cavity, with which the pyloric portion communicates by a very small orifice, and there is another orifice nearly of the same size, between this cavity and the intestine. The upper part of the duodenum is smooth, and the gall ducts open into it by a long nipple-like projection, and just below this the spiral valve has its origin, as in other sharks.

The valvular intestine, from the closeness of the turns of the valvular part, is so firm and compact, that when placed on its end, it stood upright like a cask. It is about four feet long, and ten inches in diameter, terminating at its lower end in the appearance of a rose, which is so remarkable, that it is represented in the annexed figure. Below this the rectum begins, which in this instance was two feet long. Behind the rectum, and loosely attached to the spine, is an oval bag, the coats of which are very strong: its internal membrane is reticulated, forming very deep folds, and there is a long narrow duct leading from its cavity into the rectum. This bladder

contains a dark-coloured glary fluid, and this is common to the shark tribe, but the use of such a secretion is not at present known.

The spleen in all respects resembles that of the blue shark formerly described.

The pancreas is situated in the angle between the pylorus and duodenum. It was so much broken, that its shape could not be ascertained; but its substance is composed of a soft whitish mass, intermixed with roundish bodies of a firmer texture.

The liver consists of two lobes nearly equal in size. They occupy the anterior part of the belly, from below the gills to the rectum. It yielded about three hogsheads of oil. No gall-bladder was discovered; and as a chord (like a navel string) consisting of twelve hepatic ducts passed from the liver to the duodenum, there is reason to believe that this fish has no gall-bladder. There is none in the piked whale. On cutting into the liver, the blood-vessels were found to be so large, that they readily admitted a man's arm, and on pulling them the substance of the liver was readily torn in the direction of the smaller branches, which went off at right angles to the central trunk, as regularly as those of the gills.

The kidnies are long narrow bodies, lying on each side of the spine, and extending along the whole length of the abdomen. The ureters run along their inner edge, and terminate in an oval cavity just within the verge of the anus, which has an imperfect septum separating it into two parts, the ureters opening on the opposite sides of this septum, this cavity must therefore be considered as the urinary bladder.

The testicles are situated immediately behind the origin of

the two lobes of the liver. They are oval, flattened, pulpy bodies. The epididymis nearly surrounds the testicle, and then forms the *vas deferens*, which makes many close turns upon itself, and passes downwards, adhering to the anterior surface of the kidney, in which it is in some measure embedded: the lowest part of the vas deferens, for three feet in length, becomes very large, and has no convolutions. The vasa deferentia contained a substance like thin starch, broken down into small rounded portions, mixed with a thinner fluid. These ducts were so large, as readily to allow a man's arm to be introduced up to the shoulder. Each vas deferens terminates by a small contracted orifice in the urinary bladder, one on each side, so that this bladder is both a reservoir for the urine and semen. The bladder in the male opens externally by an infundibular process, which constitutes the penis.

On each side of the anus, within its verge, near the root of the penis, is an oblique aperture, communicating freely with the cavity of the abdomen.

From the account which has been given, the Squalus maximus appears in many respects to be similar in its structure to the shark, but it differs essentially from it in the form of the stomach, and in that respect forms an intermediate link between the shark and whale. It probably lives on nearly the same kinds of food as the whale.

The sharks form a tribe of such extent, that from what we already know of their internal structure, they may be subdivided into many genera, making with the rays and scates, so many links between the whales and fishes, properly so called. The stomach and organs of generation are the parts in which the structures are most essentially different. In the

dog-fish, the stomach in its form is neither like that of the Squalus maximus, nor blue shark. I have given a drawing of it, as it appears to form a link between the two. The structure of the ovum, and the mode of hatching it is very different in the dog-fish from that of many other sharks; as I have had opportunities of investigating the mode of generation in that species, I shall make it the subject of a future communication.

I cannot close the present paper without mentioning, that nearly about the same period, two other Squali of large dimensions were thrown upon our coast. The probable cause of this event, is the season being uncommonly boisterous and tempestuous. On the 3d of January, 1809, a fish was thrown ashore at Penrhyn, in Cornwall. On hearing of it from a person on the spot, I sent down a drawing of the subject of this paper to compare with it, and the fish proves to be of the same species, and a male, measuring thirty-one feet in length.

The other was thrown ashore on the 7th of October, 1808, at Rothiesholm, an estate of GILBERT MEASON, Esq. in Stronsay, one of the Orkney isles. It had been seen lying on some sunken rocks, eleven days before, was in a half putrid state, and the sea fowl were in great numbers feeding upon it. Those who saw it, reported that the skin was rough in one direction, and smooth like satin in the other. At the time of its being examined, the skin and a great many other parts of the fish were wanting.

Mr. MEASON, with a zeal for science which does him infinite credit, upon hearing the strange accounts which were given of this sea monster, got his brother, MALCOLM LAING, Esq.

and Dr. GRANT, an eminent physician (both justices of the peace), to take depositions on the spot, from those persons who had seen the fish, that its real appearance might be ascertained. This examination, however, did not take place till six weeks after the fish was thrown ashore.

These depositions were sent to Sir JOSEPH BANKS, who put them into my hands.* I also received, a short time after, from my friend Mr. LAING, in consequence of a request I made for that purpose, that part of the skull, which contained the brain, the upper jaw having been separated from it, a considerable number of the vertebræ of the back united together by their natural attachments, a portion of one of the pectoral fins, with the cartilages that unite it to the spine, and a long and short cartilage forming the support of one of the gills. On comparing these different parts, with those of the Squalus maximus, they were found to agree, not only in their form, but also in their dimensions. This led to the opinion of the fish being a Squalus, a very different one from what was formed by those who saw it in the mutilated state in which it was thrown ashore, and who called it a *sea snake*. In the different depositions, several parts are accurately described, such as the valvular intestine, which was taken for the stomach, and the bristles of the mane, which are described as ligamentous fibres: one of them is in my possession, and is of the same kind with the fibres forming the margin of the fins of the Squalus maximus. The drawing that was made from memory, and which I have annexed, will enable me in a few words to point out how much, in some things, those who saw the fish adhered to

* The depositions are very long, and exceedingly minute; they are preserved in the Board-book of the Royal Society.

truth, and in others allowed their imaginations to supply deficiencies, for one of them declared, with confidence, that the drawing was so exact a representation of what he had seen, "that he fancied he saw the beast lying before him, at a dis-"tance, on the beach."

The drawing is correct in the representation of the head and anterior part of the fish, from which the skin, the upper and lower jaw, the gills, and gullet, had been separated by putrefaction; and when we consider that the liver and the other viscera were all destroyed, except the valvular intestine, which was taken away by the observers, the size of the body that remained would be nearly in proportion with the drawing. The legs are tolerably exact representations of the holders in the male Squalus maximus, described in a former part of this paper, and therefore are not imaginary, only that four have been added which did not exist. This is satisfactorily determined by the pectoral fin, which is preserved, having no resemblance to them. The mane, they said, was composed of ligamentous fibres, one of which was sent to London; this corresponds, in its appearance, with the fibres that form the termination of the fins and tail of the Squalus maximus, such an appearance therefore was seen, but could only be met with in the place of the two dorsal fins, instead of being continued along the back, as in the drawing. The contortions towards the tail are such, as the intervertebral joints could not admit of, they are therefore imaginary.

It is said, two different persons measured the fish; one by fathoms, the other by a foot rule, and that it was fifty-five feet long. Their accuracy is at least doubtful, as the parts that are preserved correspond with those of a fish about

thirty feet long, and it is rendered still more so, as the person who gives the length in fathoms, says, he saw at that time the six legs, the two foremost being larger than the hinder ones, and the lower joint more rounded from the body to the toes. The pectoral fin, which is preserved, proves this declaration to be incorrect: the person who measured the fish with a foot rule, declares the length, from the hole in the head to the beginning of the mane, to be exactly fifteen feet, which is probably correct, since a Squalus of about thirty-six feet long would measure, from the fore part of the skull to the dorsal fin, about fifteen feet; but the other measurement must be questionable.

It is deserving of remark, that there is no one structure represented in this drawing, which was not actually seen. The skeleton of the holders corresponds with the legs in the drawing, the margin of the dorsal fin in a putrid state with the mane; so that the only errors are in the contortions towards the tail, the length of the fish, and the number of the holders, which were mistaken for legs:* and when we recollect that the drawing was made from memory six weeks after the fish had been seen by those, who describe it, during which interval it had been their principal subject of conversation, we may conclude that so extraordinary an object, as the mutilated fish must appear when believed to be a perfect one, would, in their different discourses, have every part exagge-

* This mistake of the holders of the male shark for legs, has been frequently made. There is a drawing in Sir JOSEPH BANKS's library, sent from Ireland, in which the fish is represented walking like a duck, with broad webbed feet. The skin of a male Squalus maximus was exhibited in London some years ago distended by means of hoops, and the holders were shown as its legs, on which it occasionally walked.

rated, and it is only remarkable that the depositions kept so close to the truth as they have done.

It is of importance to science, that it should be ascertained, that this fish is not a new animal unlike any of the ordinary productions of nature, and we are indebted to the zeal and liberality of Mr. MEASON and Mr. LAING, who have collected a sufficient body of evidence to enable me to determine that point, and prove it to be a Squalus, and the orifice behind the eye, which communicates with the mouth met with in the skull, renders it very probable that it is a Squalus maximus.

This opinion is further confirmed by the Squalus maximus, known by the name of the basking shark, being frequently seen upon the coast of Scotland.

That a fish so common in the northern seas, containing a large quantity of oil, should have been so rarely caught, and indeed that it should not, as well as the whale, become an object of the Greenland fishery, appears, on the first view, not easily accounted for; but Sir JOSEPH BANKS has thrown out a suggestion which satisfactorily explains it. The whale, when struck, descends towards the bottom of the sea, but is soon obliged to rise to the surface to breathe, which enables the fishermen to follow, and prevent the breaking of their line; but the Squalus maximus, as it breathes water, has no occasion to return to the surface, and will always carry off the line.

EXPLANATION OF THE PLATES.

PLATE VI.

Fig. 1. An engraving of the Squalus maximus caught at Hastings in 1808.

a. A projecting body peculiar to the male. There is a pair of these, by means of which the female is held in the act of copulation.

Fig. 2. An exact copy of the drawing sent up to Sir JOSEPH BANKS from the Orkneys, of a Squalus in a mutilated state, without the skin or viscera, thrown ashore upon that coast, which being mistaken for a perfect animal, unlike any thing at present known, it was supposed to be a sea snake.

a. The skull, from which the upper and lower jaw had been separated by putrefaction.

b. The orbit.

c c. The spine of the fish surrounded by muscles, the gills and gullet having been separated by putrefaction.

d d. The dorsal fin, which in a half putrid state puts on a shaggy appearance, the ligamentous fibres of which the broad part is composed being separated from one another; this was mistaken for a mane, and, from want of accurate observation, was continued on to the tail, although it could only be seen in the situation of the two dorsal fins.

e e, e e, e e. The holders of the male, which are represented with tolerable accuracy as they appear in a half putrid state, but two only are met with in nature in the situation in which they are seen in Fig. 1, there they have a different appearance, the internal parts being concealed by the common integuments.

f f f. Contortions which the structure of the intervertebral substance of the fish rendered it impossible for the spine to make, and therefore could not have been seen. These contortions so represented, render it highly probable that the account of PONTOPPIDAN's sea snake had been read by the

Fig.

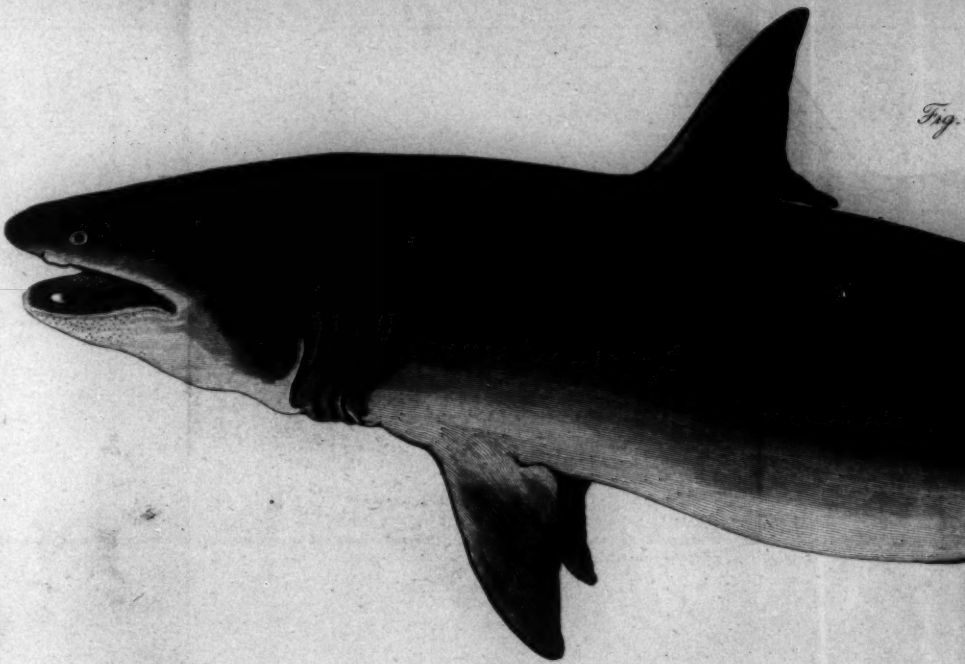
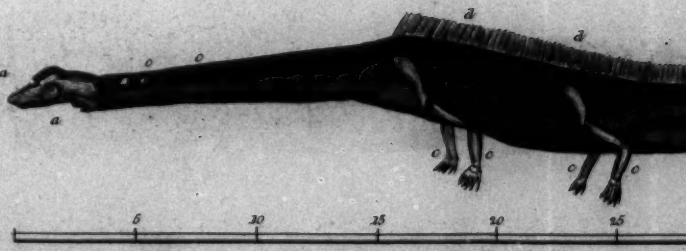


Fig.



Scale Half an Inch to a Foot.

Fig. 1.

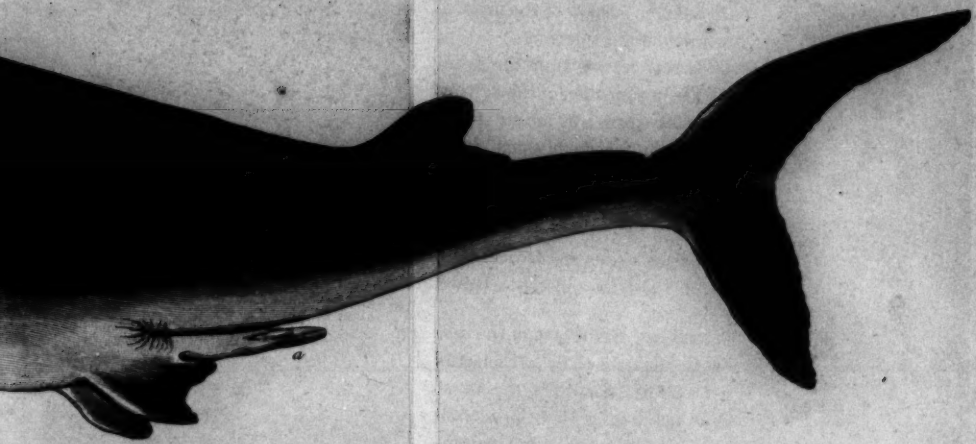
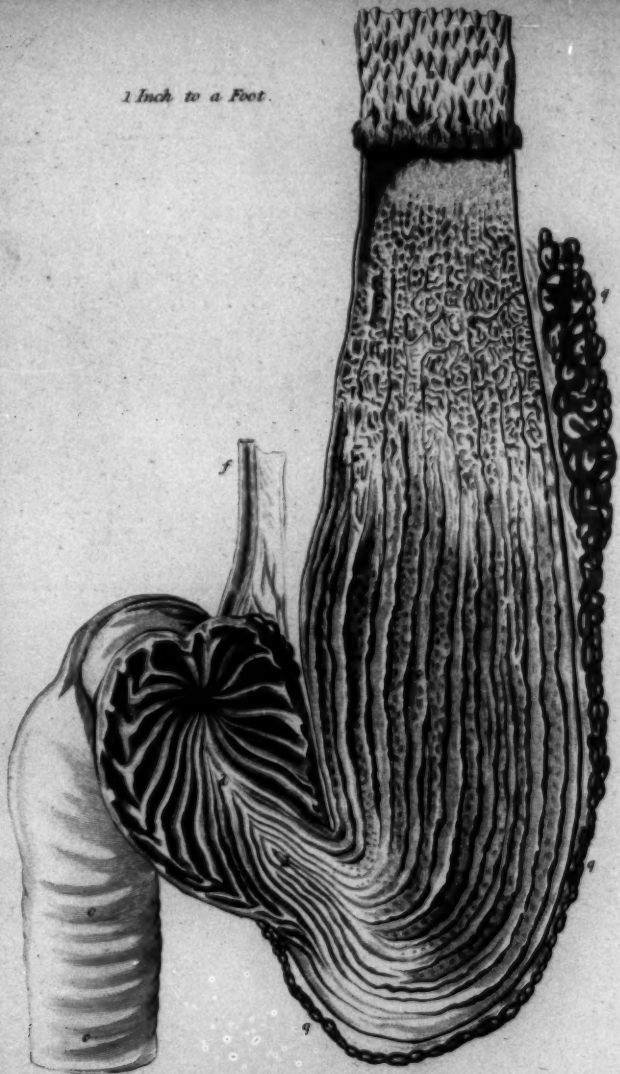


Fig. 2.

30 35 40 45 50 55 Feet.

1 Inch to a Foot.



W.m Clift del.

P. Basire sc.

Fig. 1.

2 Inches to a Foot.

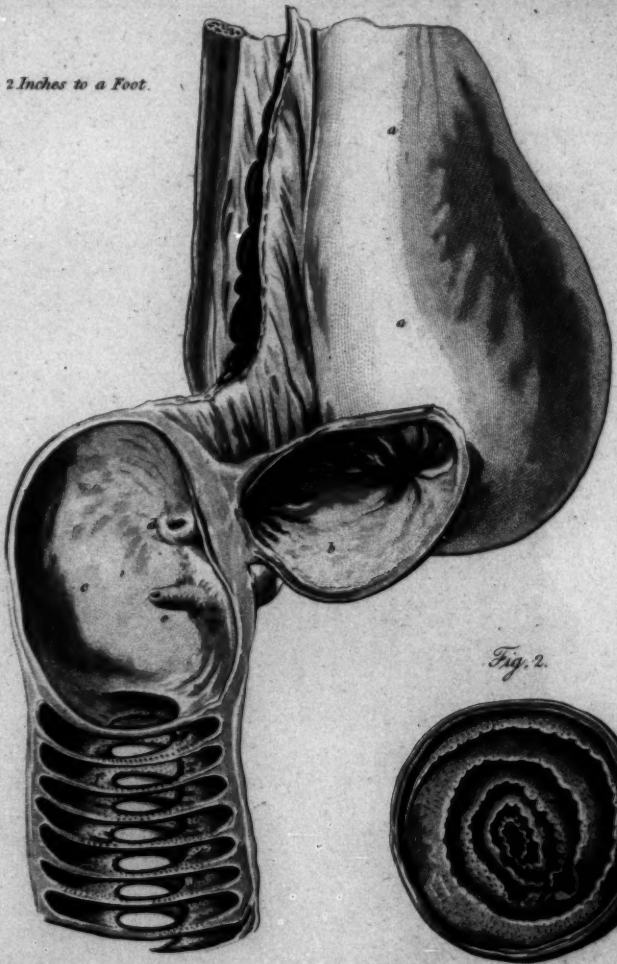


Fig. 2.

Wm. Clift del.

J. Basire sc.

spectators of this fish, in the interval of time between their seeing it and their depositions being taken.

Plate VII.

The stomach of the Squalus maximus laid open to shew the appearance of its internal structure.

a. The internal membrane of the œsophagus exposed to view.

b. The termination of the œsophagus in the stomach by a loose kind of fringe.

cc. The cardiac portion of the stomach, the upper part of which is slightly honey-combed; the lower having more of a rugous, or plicated appearance.

dd. The pyloric portion, the coats of which are very strong, and the internal rugæ very thick. The opening at the pylorus is very small, as in the whale tribe.

ee. The external surface of the valvular intestine.

f. The ducts of the liver passing down in the form of a broad band to the duodenum.

ggg. The spleen.

Plate VIII.

Fig. 1. Shews the internal surface of a small cavity interposed between the pyloric portion of the stomach and the intestine.

aa. The external surface of the pyloric portion of the stomach.

b. The cavity, with which it communicates, laid open, exposing the small opening between them, and another opening leading to the intestine.

c. The cavity of the intestine before the valvular structure begins.

d. The orifice leading into it from the cavity next the stomach.

e. The opening of the ducts from the liver.

f. The valvular portion of the intestine.

g. A portion of the spleen.

h h. The ducts passing from the liver.

Fig. 2. The termination of the valvular portion of the intestine in a rose-like form; by means of this structure the aliment is prevented from too readily making its escape.

PLATE IX.

The stomach of the common dog-fish laid open, to shew the difference of its internal structure from that of the Squalus maximus, and also the difference between it and that of the common shark.

a. The internal membrane of the œsophagus.

b. The termination of the œsophagus.

c c. The cardiac portion of the stomach, the superior part of which is plicated, and the inferior slightly honey-combed, the very reverse of what is met with in the Squalus maximus.

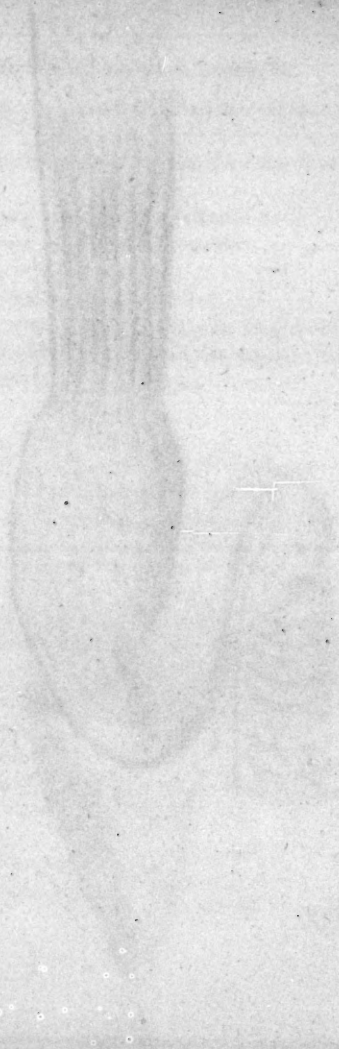
d. The pyloric portion.

e. The pylorus,

f. A small space between the pylorus and the intestine, bearing a faint resemblance to the cavity in the Squalus maximus, but so slight, as only to be detected when the fish is in a very recent state.

g. The beginning of the intestine.

h. The valvular portion.



XIII. *On an Improvement in the Manner of dividing astronomical Instruments.* By Henry Cavendish, *Esq. F. R. S.*

Read May 18, 1809.

THE great inconvenience and difficulty in the common method of dividing, arises from the danger of bruising the divisions by putting the point of the compass into them, and from the difficulty of placing that point mid-way, between two scratches very near together, without its slipping towards one of them, and it is this imperfection in the common process, which appears to have deterred Mr. TROUGHTON from using it, and thereby gave rise to the ingenious method of dividing described in the preceding part of this volume. This induced me to consider, whether the abovementioned inconvenience might not be removed, by using a beam compass with only one point, and a microscope instead of the other; and I find, that in the following manner of proceeding, we have no need of ever setting the point of the compass into a division, and consequently that the great objection to the old method of dividing is entirely removed.

In this method, it is necessary to have a convenient support for the beam compass: and the following seems to me to be as convenient as any. Let CCC (Fig. 1.) be the circle to be divided, BBB a frame resting steadily on its face, and made to slide round on it with an adjusting motion to bring it to any required point: $d\delta$ is the beam compass, having a point near

δ, and a microscope m made to slide from one end to the other. This beam compass is supported at d, in such manner as to turn round on this point as a centre, without shake or tottering; and at the end δ it rests on another support, which can readily be lowered, so as either to let the point rest on the circle, or to prevent its touching it. It must be observed, however, that as the distance of d from the center of the circle must be varied, according to the magnitude of the arch to be divided, the piece on which d is supported had best be made to slide nearer to, or further from, the center; but the frame must be made to bear constantly against the edge of the circle to be divided, so that the distance of d from the center of this circle, shall not alter by sliding the frame.

This being premised, we will first consider the manner of dividing by continued bisection. Let F and f be two points on this limb which are to be bisected in φ. Take the distance of the microscope from the point nearly equal to the chord of $f\varphi$, and place d so that the point and the axis of the microscope shall both be in the circle in which the divisions are to be cut. Then slide the frame BBB till the wire of the microscope bisects the point F; and having lowered the support at δ, make a faint scratch with the point.

Having done this, turn the beam compass round on the center d till the point comes to D, where it must rest on a support similar to that at δ; and having slid the frame till the wire of the microscope bisects the point f, make another faint scratch with the point, which if the distance of the microscope from the point has been well taken, will be very near the former scratch; and the point mid-way between them will be the accurate bisection of the arch Ff; but it is unnecessary,

and better not to attempt to place a point between these two scratches.

Having by these means determined the bisection at φ, we must bisect the arches $F\varphi$ and $f\varphi$ in just the same manner as before, except that the wire of the microscope must be made to bisect the interval between the two faint scratches, instead of bisecting a point.

It must be observed, that when the arch to be bisected is small, it will be necessary to use a bent point, as otherwise it could not be brought near enough to the axis of the microscope; and then part of the rays, which form the image of the object seen by the microscope, will be intercepted by the point; but I believe, that by proper management this may be done without either making the point too weak, or making the image indistinct; but if this cannot be done, we may have recourse to Mr. TROUGHTON's expedient of bisecting an odd number of contiguous divisions.

It must be observed too, that in the bisections of all the arches of the same magnitude, the position of the point d on the frame remains unaltered; but its position must be altered every time the magnitude of the arch is altered.

It is scarcely necessary to say, that the bisections thus made are not intended as the real divisions, but only as marks from which they are to be cut. In order to make the real divisions, the microscope must be placed near the point, and the support d must be placed so that $d\delta$ shall be a tangent to the circle at δ. The wire of the microscope must then be made to bisect one of these marks, and a point or division cut with the point, and the process continued till the divisions are all made.

It is plain that in this way, without some further precaution,

we must depend on the microscope not altering its position in respect of the point during the operation; for which reason I should prefer placing the axis of the microscope at exactly the same distance from the center of motion *d*, as the point; but removed from it sideways, by nearly the semi-diameter of the object glass; so that having made the division, we may move the beam compass till the division comes within the field of the microscope, and then see whether it is bisected by the wire, and consequently see whether the microscope has altered its place.

In the operation of bisection, as above described, it may be observed, that if the two scratches are placed so near together, that in making the second the point of the compass runs into the burr raised by the first, there seems to be some danger that the point may be a little deflected from its true course; though in Bird's account of his method, I do not find that he apprehends any inconvenience from it. One way of obviating this inconvenience, if it does exist, would be to set the beam compass not so exactly to the true length, as that one scratch should run into the burr of the other; but as this would make it more difficult to judge of the true point of bisection, perhaps it might be better to make one scratch extend from the circle towards the center, and the other from it.

It is clear, that the entire arc of a circle cannot be divided to degrees, without trisection and quinquesection; and I do not know whether our artists have recourse to this operation, or whether they avoid it by some contrivance similar to Bird's, namely, that of laying down an arch capable of continued bisection; but if the method of quinquesection is preferred, it may be performed by either of the three following methods:

First Method.

Let $a\,a$ (Fig. 2) be the arch to be quinquesected. Open the beam compass to the chord of one fifth of this arch; bring the microscope to a, and with the point make the scratch f; then bring the microscope to f, and draw the scratch e; and in the same manner make the scratches d and b. Then turn the beam compass half round, and having brought the microscope to α, make the scratch β; and proceeding as before, make the scratches δ, ι and ϕ. Then the true position of the first quinquesection will be between b and β, distant from β by one fifth of $b\,\beta$; and the second will be distant from δ by two fifths of $d\,\delta$, and so on.

Then, in subdividing these arches, and striking the true divisions, the wire of the microscope, instead of bisecting the interval between the two scratches, must be brought four times nearer to β than to b. But in order to avoid the confusion which would otherwise proceed from this, it will be necessary to place marks on the limb opposite to all those divisions, in which the interval of the scratches is not to be bisected, shewing in what proportion they are to be divided; and these marks should be placed so as to be visible through the microscope, at the same time as the scratches. Perhaps, the best way of forming these marks, would be to make dots with the point of the beam compass contiguous to that scratch which the wire is to be nearest to, which may be done at the time the scratch is drawn.

Perhaps an experienced eye might be able to place the wire in the proper manner, between the two scratches, without further assistance; but the most accurate way would be to

have a moveable wire with a micrometer, in the focus of the microscope, as well as a fixed one; and then having brought the fixed wire to b, bring the moveable one to β, and observe the distance of the two wires by the micrometer; then reduce the distance of the two wires to one fifth part of this, and move the frame till the moveable wire comes to β, and then the fixed wire will be in the proper position, that is four times nearer to β than to b.

It will be a great convenience, that the moveable wire should be made in such manner, as to be readily distinguished from the fixed, without the trouble of moving it.

In this manner of proceeding, I think a careful operator can hardly make any mistake: for if he makes any considerable error in the distance of the moveable wire from the fixed, it will be detected by the fixed wire not appearing in the right position, in respect of the two scratches; and as the mark is seen through the microscope, at the same time as the scratches, there is no danger of his mistaking which scratch it is to be nearest to, or at what distance it is to be placed from it.

To judge of the comparative accuracy of this method with that of bisection, it must be considered that the arches $\alpha\beta$, $\beta\delta$, &c. though made with the same opening of the compass, will not be exactly alike, owing partly to irregularities in the brass, and partly to other causes. Let us suppose, therefore, that in dividing the arch $a\alpha$ into five parts, the beam compass is opened to the exact length, but that from the abovementioned irregularities the arches $\alpha\beta$, $\beta\delta$, $\delta\varepsilon$, and $\varepsilon\varphi$ are all too long by the small quantity ε, and that the arches af, fe, ed, and db are all too short by the same quantity, which is the supposition the most unfavourable of any to the exactness of

the operation; then the error in the position of $\beta = \epsilon$, and the point b errs 4ϵ in the same direction, and therefore the point assumed as the true point of quinquesection, will be at the distance of $\frac{3\epsilon}{5}$ from β, and the error in the position of this point $= \epsilon \times 1\frac{1}{5}$.

By the same way of reasoning, the error in the position of the point taken between d and $\delta = \epsilon \times 2\frac{2}{5}$.

In trisecting the error of each point $= \epsilon \times 1\frac{1}{5}$; and in bisecting, the error $= \epsilon$; and in quadrisecting, the error of the middle point $= 2\epsilon$.

It appears therefore that in trisecting, the greatest error we are liable to does not exceed that of bisection in a greater proportion than that of 4 to 3; but in quinquesecting the error of the two middle points is $2\frac{2}{5}$ times greater than in bisecting. It must be considered, however, that in the method of continued bisection, the two opposite points must be found by quadrisection; and the error of quinquesection exceeds that of quadrisection in no greater proportion than that of six to five; so that we may fairly say, that if we begin with quinquesection, this method of dividing is not greatly inferior, in point of accuracy, to that by continued bisection.

Second Method.

This differs from the foregoing, in placing dots or scratches in the true points of quinquesection and trisection, before we begin to subdivide. For this purpose, we must have a microscope placed as in page 224, first par. at the same distance from the center of motion as the point is; and this microscope must be furnished with a moveable wire and micrometer, as in page

226; and then having first made the fixed wire of this microscope correspond exactly with the point, we must draw the scratches b and β, d and δ, &c. as before, and bring the fixed wire to the true point of quinquesection between b and β, in the manner directed in page 226, and with the point strike the scratch or dot; and if we please, we may, for further security, as soon as this is done, examine, by means of the moveable wire, whether this intermediate scratch or dot is well placed.

The advantage of this method is, that when this is done, we may subdivide and cut the true divisions, by making the wire of the microscope bisect the intermediate scratches, instead of being obliged to use the more troublesome operation of placing it in the proper proportion of distance between the two extremes.

This method certainly requires less attention than the former, and on the whole seems to be attended with considerably less trouble; but it is not quite so exact, as we are liable to the double error of placing the intermediate point and of subdividing from it.

As in this method the intermediate points are placed by means of the micrometer, there is no inconvenience in placing the extreme scratches b and β, &c. at such a distance from each other, that the intermediate one shall be in no danger of running into the bur raised by the extremes.

Third Method.

Let $a\alpha$ (Fig. 3) be the arch to be quinquesected; lay down the arches ab, bd, and de, as in the first method; then turn the beam compass half round, and lay down the arches $\alpha\beta$

and $\beta\delta$; then, without altering the frame, move the moveable wire of the microscope till it is four times nearer to δ than to e, and, having first rubbed out the former scratches, lay them down again with the compass thus altered; but as this method possesses not much, if any, advantage over the second, in point of ease, and is certainly inferior to it in exactness, it is not worth while saying any thing further about it.

It was before said,* that the center of motion of the beam compass is to be placed, so that the point and axis of the microscope shall both be in the circle in which the divisions are made; but it is necessary to consider this more accurately. Let A δ (Fig. 4) be the circle in which the scratches are to be made, δ the point of the beam compass, which we will suppose to be exactly in this circle, d the center on which it turns, and M m the wire in the focus of the microscope, and let m be that point in which it is cut by the circle; and let us suppose that this point is not exactly in the line $d\delta$, then, when the beam compass is turned round, the circle will cut the wire in a different point μ, placed as much on one side of $d\delta$, as m is on the other, so that if the wire is not perpendicular to $d\delta$, the arch set off by the beam compass, after being turned round, will not be the same as before; but if it is perpendicular, there will be no difference; for which reason, care should be taken to make the wire exactly perpendicular to $d\delta$, which is easily examined by observing whether a point appears to run along it, while the beam compass is turned a little on its center. It is also necessary to take care that the point δ is in the arc of the circle, while the bisection is observed by the microscope, which may most conveniently be obtained, by placing a stop

* Page 122.

on the support on which that end of the beam compass rests. If proper care, however, is taken in placing the wire perpendicular, no great nicety is required either in this or in the position of *d*.

Another thing to be attended to, in making the wire bisect two scratches, is to take care that it bisects them in the part where they cut the circle; for as the wire is not perpendicular to the circle, except in very small arches, it is plain, that if it bisects the scratches at the circle, it will not bisect them at a distance from it.

There are many particulars in which my description of the apparatus to be employed will appear incomplete; but as there is nothing in it which seems attended with difficulty, I thought it best not to enter further into particulars, than was necessary to explain the principle, and to leave the rest to any artist who may choose to try it.

It is difficult to form a proper judgment of the conveniences or inconveniences of this method, without experience; but, as far as I can judge, it must have much advantage, both in point of accuracy and ease, over that of dividing by the common beam compasses; but it very likely may be thought that Mr. TROUGHTON's method, is better than either. Whether it is or is not, must be left for determination to experience and the judgment of artists. Thus much, however, may be observed, that this, as well as his, is free from the difficulty and inaccuracy of setting the point of a compass exactly in the center of a division. It also requires much less apparatus than his, and is free from any danger of error, from the slipping or irregularity in the motion of a roller; in which respect his method, notwithstanding the precautions used by

Fig. 1.

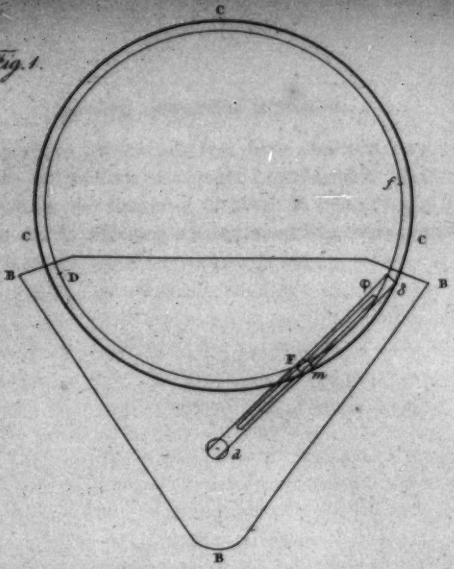
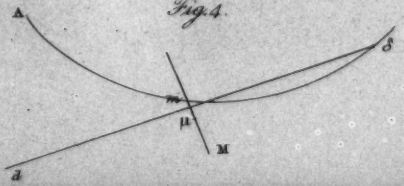


Fig. 2.

Fig. 3.

Fig. 4.



him, is perhaps not entirely free from objection; and what with some artists may be thought a considerable advantage, it is free from the danger of mistakes in computing a table of errors, and in adjusting a sector according to the numbers of that table.

XIV. *On a Method of examining the Divisions of astronomical Instruments. By the Rev.* William Lax, *A. M. F. R. S.* Lowndes's *Professor of Astronomy in the University of Cambridge. In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. Astronomer Royal.*

Read June 1, 1809.

DEAR SIR,

St. Ibbs, Aug. 27, 1808.

I AM persuaded that you must feel, in common with myself, how unpleasant it is to make use of an instrument in astronomical observations requiring extreme accuracy, whose exactness you have no adequate means of ascertaining, but are obliged to depend for it in a great measure upon the abilities and integrity of the artist. It is in vain that we observe with so much nicety, and read off with so much precision, if we are still uncertain whether there may not be an error in the instrument itself of much greater magnitude, than those which we are endeavouring to prevent; and that our best instruments must be liable to such errors, no person can possibly doubt, who has paid due attention to the sources from whence they may arise. I have estimated, as accurately as I could, the amount to which they may accumulate in BIRD's method of dividing by continual bisections, and have satisfied myself that they are much more considerable than is generally apprehended; but as I cannot obtain such precise information as I could wish, respecting the exactness with which a bisection can be performed, or a length taken from the scale of equal

parts and laid upon the instrument, I will not trouble you with the deduction which I have made. It is understood indeed, that BIRD's method is now generally laid aside, and that each artist employs one, which he considers in many respects as peculiar to himself; but I presumed that there would still be such a connection betwixt BIRD's method and those which have been substituted in its stead, as to render them in some degree liable to the same errors to which it was subject, and the reports which I have uniformly received from persons, who have had an opportunity of examining some of the modern instruments, have fully convinced me that my opinion was just. But whatever may be the nature of the methods which are now in use, or whatever their advantages over BIRD's, I never could persuade myself that it would be safe to trust to an instrument, without a previous examination. To discover the means of accomplishing this object, is what I have for some time been anxious to effect, and though I fear my endeavours have not been very successful, I will nevertheless take the liberty of presenting you with the result.

You are aware, I believe, that I use a circular instrument for observing both in altitude and azimuth, which was made for me by Mr. CARY in the Strand; that the radius of both the altitude and the azimuth circle is one foot, and that each is divided into parts containing ten minutes. The construction of this instrument does not differ materially from that of other similar instruments, with which you are well acquainted, and I shall not therefore waste your time by giving you a particular description of it. For the purpose of examining the divisions upon the two circles, I procured an apparatus to be prepared by Mr. CARY, which will be very easily explained.

To the face of the rim which surrounds the azimuth circle, and with its left end close to the stand which supports the micrometer on the east side, an arc of brass, concentric with the circle itself, and a little more than 90° in length, an inch in breadth, and one eighth of an inch in thickness, is firmly fixed by screws, so as to have the plane parallel to the plane of the circle, and a small portion of its lower surface resting upon the extreme part of the rim. The screws pass through a brass arc, which is fastened to this at right angles, and lies with its broad side against the face of the rim. Upon the first mentioned arc, a strong upright piece of brass, about six inches in length, is made to slide, the lower part of it embracing the arc as a groove and having a clamping screw underneath, for the purpose of fixing it firmly to the arc at any point required. To the top of the upright piece of brass is attached a microscope, with a moveable wire in its focus, pointing down to the division upon the circle, not directly, however, but with an inclination to the left of about 30°. This inclination is given to it, in order to make it point to the same division upon the circle, which is immediately under the micrometer itself, when it has been moved up as near to the micrometer, as it is capable of approaching. The microscope has attached to it a small graduated circle of brass, and an index, by which the seconds, and parts of a second, moved over by the wire are determined.

To the vertical circle there is likewise an arc applied, of the same length and breadth as the former, but somewhat thicker, and of a radius exceeding that of the circle by about two inches. This greater thickness is given to it, on account of its being supported in a manner which renders additional

strength necessary. It is fixed with its broad convex side downwards upon two brass pillars, screwed fast to the plane of the azimuth circle, and standing in a line parallel to the plane of the vertical circle at the distance of about four inches from it, and on the right side of the pillars which support the micrometers belonging to this circle. The pillar, to which the left end of the arc is fastened, is placed close to the lower micrometer of the vertical circle, and the other contiguous to the elevated rim, in which the divisions of the azimuth circle are cut. The right end of the arc reaches beyond this pillar about ten inches. The pillars are of such a height, and so proportioned to each other, that whilst the left end of the arc, which lies horizontally, is raised to within about two inches of the height at which the lowest point of the vertical circle is placed, the whole arc runs parallel to the circle through an extent of something more than 90°. Upon the arc a microscope, with a moveable wire in the focus, is made to slide as in the former case, and to point to the divisions upon the vertical circle, not directly, but with an inclination of about 30° to the left, in order that the same division (which is the lowest upon the circle) may be seen through it, and through the lower micrometer at the same time.

I will now proceed to shew you, in what manner the examination of the divisions upon either circle may be performed. The process is precisely the same in both cases, and will of course be described in the same words.

The first point to be examined is that of 180°, which must be done in the usual way, by bringing the points of 0 and 180° to the moveable wires of the opposite micrometers, and then turning the circle half-way round, and bisecting the points

again with the moveable wires; and lastly, taking half the difference betwixt the distances of the wires in the two positions of the circle for the error at the point of 180°. Having now bisected the point of *zero* with the moveable wire of the micrometer, which is intended to be used in the rest of the process (for we shall have no further occasion for both), we must slide the microscope along the arc, till by moving the wire a little we can bisect the point of 90°, and then the micrometer must be firmly clamped to the arc. The circle must then be turned till the point of 180° is brought to the microscope, and that of 90° to the micrometer, so that we may be able to bisect each by a slight motion of their respective wires. This being done, we must observe, from the positions of the wires, how much the interval betwixt them has increased or decreased in the measurement of the new arc; and this increase or decrease must be noted down with a + or — accordingly. In the same manner we must proceed through the remaining two arcs of 90°, observing and noting down the difference betwixt each and the original arc.

The point of *zero* must now be brought again to the micrometer, and bisected by the moveable wire, and the microscope be made to slide back along the arc, till by moving the wire a little we can bisect the point of 60°, and when this is done, the microscope must be clamped. We must then measure the arc of 60° against every succeeding arc of 60° in the circle, precisely in the same way that we measured the first arc of 90° against the other three. The arc of 45° is next to be measured against every succeeding arc of 45°, and this will complete all that is necessary to be done in the early part of the morning before the heat of the sun can have affected the

temperature of the instrument. The rest may be performed at our leisure.

You will immediately perceive the object of this kind of measurement. It enables us to determine, with any degree of accuracy that may be required, the proportion which the first and every succeeding arc of the circle, contained betwixt the micrometer and the microscope, bears to the whole circle, and of course the absolute length of the arcs themselves. Let a denote the real length of the first of these, and $\pm a'$, $\pm a''$, $\pm a'''$, &c. the difference betwixt the first and second, the first and third, &c. respectively; let A represent any other arc whose length is known, and which is a multiple of a, as marked upon the instrument, and let this multiple be expressed by n. Then will $a + (a + a') + (a + a'') + (a + a''') +$ &c. ... $(a + a'''^{\cdots\overline{n-1}}) = A$, and $a = \frac{A - a' - a'' - \ldots a'^{\cdots\overline{n-1}}}{n}$. Hence it is evident, that if there is no error committed in the measurement of any of these arcs, we shall have the value of a, and consequently of $a + a'$, $a + a''$, $a + a'''$, &c. and of any arc, comprehending any number of these, accurately determined. But if there be an error of e in the measurement of the first, of e', e'', e''', &c. in the measurement of the second, third, &c. respectively, then we shall have the following equation for determining a, viz. $a + (a + a' + e + e') + (a + a'' + e + e'') +$ &c. ... $(a + a'''^{\cdots\overline{n-1}} + e + e'^{\cdots\overline{n-1}}) = A$, and consequently a will appear to be equal to $\frac{A - a' - a'' - \ldots a'^{\cdots\overline{n-1}}}{n} \frac{-\overline{n-1}e - e' - e'' - \ldots e'^{\cdots\overline{n-1}}}{n}$, which differs from its true value by $\frac{\overline{n-1}e + e' + e'' + \ldots e'^{\cdots\overline{n-1}}}{n}$. Hence it follows, that the value of

the p^{th} arc (p being greater than unity), as deduced by this process, will differ from its true value by $\frac{\overline{n-1}.e+e'+e''+\ldots e^{\cdots\overline{p-1}}}{n}$ $+\frac{e^{\cdots p}+\ldots e^{\cdots\overline{n-1}}}{n}-e-e^{\prime\prime\cdots\overline{p-1}}$, and that if we add any number p of these arcs together, in order to determine the value of the arc which is equal to their sum, we shall have an error in this value (and the expression holds when p is unity, or the first arc only is taken) equal to $p\frac{\overline{n-1}.e+e'+e''+\ldots e^{\cdots\overline{p-1}}}{n}$ $+\frac{e^{\cdots p}+\ldots e^{\cdots\overline{n-1}}}{n}-\overline{p-1}.e-e'-e''-\ldots e^{\prime\prime\cdots\overline{p-1}}=\frac{\overline{n-p}.e-e'-e''-\ldots e^{\cdots\overline{p-1}}+p.e^{\cdots p}+e^{\cdots p+1}+\ldots e^{\cdots n-1}}{n}$.

Now, if we suppose e to be the greatest error to which we are liable in the measurement of any arc, and each of the succeeding errors to be equal to it, and likewise that $e', e'', \ldots e^{\prime\prime\cdots\overline{p-1}}$ are all negative, then it will appear that $\frac{n-p}{n}\times 2pe$ will be the greatest error that can be committed in determining the value of any arc, by adding together the values of the (p) smaller arcs of which it is compounded. For instance, if the interval betwixt the micrometer and the microscope comprehends an arc of 60°, as marked upon the instrument, and this arc is measured against every succeeding arc of 60° in the whole circle, we shall have the greatest error that can be committed in deducing the arc of 120° from the addition of the two first arcs of 60°, equal to $\frac{6-2}{6}\times 2\times 2e=2.66e$.

After these remarks, we may proceed to consider how the remaining divisions upon the circle may be examined with the least probable error, and to ascertain the amount of the greatest to which the process can in any case be liable.

Let the arc of 30° be now measured against every succeeding arc of 30° in the first, third, fourth, and sixth arcs of 60°, and let the length of each be determined from a separate comparison with the arc of 60°, in which it is comprehended, and not from a general comparison with all the four. The arc of 15° must then be measured against every succeeding arc of 15° in the first, third, fourth, sixth, seventh, ninth, tenth, and twelfth arcs of 30°, and the value of each deduced from a comparison with the arc of 30°, in which it is contained. When this is done, we shall have determined the length of every succeeding arc of 15°, of the first arcs of 30, 45, 60, 75 (= 60 + 15), 90, 105 (= 90 + 15), 120 (= 60 + 60), 135 (= 90 + 45), 150 (= 120 + 30), 165 (= 150 + 15), and 180° in each semi-circle.

We must next measure the arc of 5° against every succeeding arc of 5° in the whole circle, and deduce the values of the first, and of the sum of the first and second, in each succeeding arc of 15° from a comparison with the arc of 15° in which they are contained. We must then proceed to determine the values of the first arc of 3° in each 15°, and of its multiples the arcs of 6, 9, and 12°. We must also put down the value of the last arc of 3° in each arc of 15°, and then deduce the values of the first and last arcs of 1° in each arc of 15°, from a comparison with the arc of 3° in which they are respectively contained.

We shall now have measured in each arc of 15° the first arcs of 1, 3, 5, 6, 9, 10, 12°, and by taking the last arc of one degree, which has likewise been determined, from the arc of 15°, we shall obtain the first arc of 14°. The first 7° of this arc being measured against the second, we ascertain the value of the first 7°; and then, by measuring the first 4° of the re-

maining arc of 8° against the second, we shall get the value of the first 4°, which added to the arc of 7°, before determined, will give us the length of the first arc of 11°. The first 2° of the remaining arc of 4° must then be measured against the second, and we shall get the value of the first 2°, and by adding this arc to the arc of 11°, we shall obtain the value of the arc of 13°. By taking away the first arc of 1° from the arc of 15°, we get the remaining arc of 14°, and then having determined the length of the first 7° of this arc, by measuring them against the second, we must add it to the arc of 1°, and we shall obtain the arc of 8°. The length of the first 4° of this arc will then be easily known, by measuring them against the second, as will afterwards that of the first 2° in the arc of 4° itself, by measuring them against the second in the same arc.

We have still to ascertain the lengths of all the first arcs of 10, 20, 30, 40, and 50 minutes contained in each degree, for I shall only consider the case in which the circle is divided into parts of 10 minutes. Now the length of the first arc of 30′ will be obtained by measuring it against the second, and the lengths of the first and second arcs of 20′ (whose sum will give the arc of 40′) by measuring the first against each of the remaining arcs. The length of the third arc of 20′ must likewise be put down, and then the first arc of 10′ being measured against the second of the arc of 20′, in which it is included, and also against the two arcs of 10′ contained in the last arc of 20′, its own value, and that of the last 10′ in the degree will be determined from a comparison with the arcs of 20′, in which they are respectively comprehended. The length of this last arc of 10′ being taken from that of the whole degree, will give us the length of the first 50′, and complete the operation.

In order to ascertain the greatest possible error to which we are liable in the examination, let s denote in parts of a second the greatest that can be committed in bisecting any point upon the limb; then, since this error may occur at each end of the arc, it is evident that e in the expression deduced above ($\frac{n-p}{n} \times 2pe$) will become $2s$, and the expression itself $\frac{n-p}{n} \times 4ps$. Hence the possible error will be $\frac{2-1}{2} 4s = 2s$ at 180°; $\frac{2s}{2} + \frac{2-1}{2} \times 4s = 3s$ at 90°; $\frac{2s}{3} + \frac{3-1}{3} \times 4s = 3.33s$ at 60°; $\frac{2}{3} \times 2s + \frac{3-2}{3} \times 4 \times 2s = 4s$ at 120°. The greatest error must therefore lie betwixt 90 and 120°, and nearer to the extremity of the latter than of the former arc. At 105° it will be $5.50s$; at 111° it will be $5.50s - \frac{2}{5}.1,5s + \frac{5-2}{5} \times 4 \times 2s = 9,70s$; and at 111° 10′ it will be $9,70s - \frac{1}{5}.1,04s$ (the excess of the error at 111° above that at 112°) $+ 3.33s = 12.86s$, which will be found to be the greatest error betwixt 105 and 120°, and of course the greatest in the first semi-circle. In the other semi-circle, the process being the same, the possible errors must necessarily be the same at the same distances from the first point, reckoning the contrary way upon the circle.

The magnitude of the quantity s will of course vary upon circles of the same radius, according to the excellence of the glass employed, and the accuracy of the examiner's eye. It will seldom, however, exceed one second upon a circle, whose radius is one foot; and in general it will not amount to so much. I find that I can read off, to a certainty, within less than three fourths of a second, and hence I conclude, that I could examine the divisions of my circle without being liable to a greater error than 9.63 seconds, and those of a circle of

three feet radius without the risk of a greater error than 3,21 seconds.

To those people who are accustomed to entertain such exalted notions of the accuracy with which astronomical instruments can with a certainty be divided, this error, I dare say, will appear very considerable; but for my part, I am perfectly satisfied that it bears but a small proportion to the accumulated error which may take place, in spite of the utmost vigilance of the artist, in an instrument divided according to any method which has hitherto been made public. I need not, however, remark upon the very great improbability that the error of examination should ever attain, or approach, to its extreme limit, as this must be sufficiently obvious to any person who is in the least degree conversant with the doctrine of chances; but it may be proper to observe, that we have it in our power (and in this respect the examiner possesses a most important advantage over the divider of an instrument) to diminish its probable amount, as much as we please, by bringing the moveable wires of the micrometer and microscope several times to bisect their respective points in the measurement of every arc, and taking a mean of the different *readings-off* for the true position of the wire at the real bisection of the point. The wire may be moved in this manner eight or ten times at each point (if such a degree of caution should be thought necessary), and the mean taken in little more than a minute, so that the time of performing the work will not be so much increased, as might perhaps have been apprehended, and when it is completed, we may reasonably presume that the distance of every point from zero (whilst the temperature of the circle continues uniform) will have been

determined with sufficient exactness for every practical purpose.

Of the time necessary for the examination, a pretty correct idea may be formed by considering how many measurements are required, and allowing about a minute and a half for each; *i. e.* a quarter of a minute for bringing the extreme points of the arc to the micrometer and the microscope, and a minute and a quarter for making the several bisections. Now, in dividing the whole circle into arcs of 15° each, it will appear that forty-four measurements must be performed; and to examine every point in each arc of 15°, there will be 161 required, making in all 3908 measurements; and consequently the time necessary for completing the whole work will be 5862 minutes, or about 98 hours.

The time and labour required for this examination are, no doubt, very considerable; but it ought to be recollected, that it will render any great degree of precision, in dividing the instrument, totally unnecessary. Whoever indeed employs this method of examinination will be virtually the divider of his own instrument, and all that he will ask of the artist, is to make him a point about the end of every five or ten minutes, whose distance from zero he will determine for himself, and enter in his book to be referred to when wanted. We may likewise observe that by this examination we shall not only be secured against the errors of division, but against those which arise from bad centering, and from the imperfect figure of the circle, and which in general are of too great a magnitude to be neglected.

It will, I dare say, have occurred to you, that whenever we are desirous that an observation should be particularly exact,

we may guard it against the effects of unequal expansion or contraction in the metal, by means of the apparatus which I have described: for we have only to measure the arc which has been determined by the observation against the whole circle, or against the multiple of it, which approaches nearest to the circle, and from thence to deduce its value in the manner explained above, and we shall either have entirely excluded the error which we apprehended, or have rendered it too small to be of any importance. Suppose, for instance, that the arc determined by the observation was 48°; then by measuring it against the whole circumference increased by an arc of 24°, we shall obtain a result free from any greater error of unequal temperature, than one eighth of the increase or decrease of this arc of 24° beyond a due proportion to that of the circle itself.

This expedient gives us all the advantages of the French circle of repetition, without the inconvenience arising from being obliged to turn the instrument, and move the telescope, so many times in the course of the observation. Nay, I am persuaded, that the result may be made more accurate in this way, than by the French method, because not only can the object be more frequently observed, but the contacts or bisections, it may be presumed, will be more exact when the observer is not disturbed by the hurry attendant upon the use of the repeating circle; and with respect to any error in the instrument, from whatever cause it may arise, it will be as effectually excluded by the process which I recommend, as by moving the telescope round the circle. Besides this method is applicable either to the azimuth or altitude circle, or indeed to any circle which turns upon its own axis, whereas

the French method can never be applied to the azimuth circle, nor to any other circle which does not turn both upon its own axis, and upon one which is perpendicular to it.

After all, however, it is possible that the process which I have been explaining to you may be no new discovery, and that you may be already acquainted with it. If this should be the case, you will be kind enough to inform me. At any rate, indeed, I should esteem myself greatly obliged, if you would favour me with your sentiments upon the subject, as soon as you can do it with perfect convenience to yourself.

I am, Dear Sir,

yours, &c.

WILLIAM LAX.

XV. *On the Identity of Columbium and Tantalum.* By William Hyde Wollaston, *M. D. Sec. R. S.*

Read June 8, 1809.

WITHIN a short time after the discovery of columbium by Mr. HATCHETT in 1801,* a metallic substance was also discovered in Sweden by M. EKEBERG,† differing from every metal then known to him; and accordingly he described the properties by which it might be distinguished from those which it most nearly resembled. But although the Swedish metal has retained the name of Tantalum given to it by M. EKEBERG, a reasonable degree of doubt has been entertained by chemists, whether these two authors had not in fact described the same substances; and it has been regretted that the discoverers themselves, who would have been most able to remove the uncertainty, had not had opportunities of comparing their respective minerals, or the products of their analyses.

As I have lately obtained small specimens of the two Swedish minerals, tantalite and yttro-tantalite, from which I could obtain tantalum, and was very desirous of comparing its properties with those of columbium, Mr. HATCHETT very obligingly furnished me with some oxide of the latter, which remained in his possession.

* Phil. Trans. for 1802.

† *Vetenskaps Academiens Handlingar*, 1802, p. 68.—*Journal des Mines*, Vol. XII. p. 245.

The resemblance was such in my first trials, as to induce me to endeavour to procure a further supply of columbium, and by application to the Trustees of the British Museum, I was allowed to detach a few grains from the original specimen analysed by Mr. HATCHETT.

Notwithstanding the quantity employed in my analyses was thus limited, I have, nevertheless, by proportionate economy of the materials, been enabled to render my experiments sufficiently numerous, and have found so many points of agreement in the modes by which each of these bodies can or cannot be dissolved or precipitated, as to prove very satisfactorily that these American and Swedish specimens in fact contain the same metal; and since the re-agents I have employed are in the hands of every chemist, the properties which I shall enumerate are such as will be most useful in the practical examination of any other minerals in which this metal may be found to occur.

In appearance the columbite is so like tantalite, that it is extremely difficult to discern a difference that can be relied upon. The external surface, as well as the colour and lustre of the fracture, are precisely the same; but columbite breaks rather more easily by a blow, and the fracture of it is less uniform, appearing in some parts irregularly shattered; nevertheless, when the two are rubbed against each other, the hardness appears to be the same, and the colour of the scratch has the same tint of very dark brown.

By analysis also, these bodies are found to consist of the same three ingredients; a white oxide, combined with iron and manganese.

Either of these minerals, when reduced to powder, is very

readily acted upon by potash; but as the iron contained in them is not affected by alkalies, it appeared better to add a small proportion of borax.

Five grains of columbite being mixed with twenty-five grains of carbonate of potash and ten grains of borax, were fused together for a few minutes, and found to be perfectly incorporated. The colour was of a deep green, from the quantity of manganese present. The mass when cold could be softened with water, and a portion of the oxide could be so dissolved; but it seemed preferable to employ dilute muriatic acid, which, by dissolving all other ingredients excepting columbium, left the oxide nearly white, by the removal of iron and manganese that been combined with it.

The muriatic solution having been poured off and neutralized with carbonate of ammonia, the iron was then separated by succinate of ammonia; after which the manganese was precipitated by prussiate of potash.

The products thus obtained from five grains of columbite, after each had been heated to redness, were nearly,

White oxide	-	4 grains
Oxide of iron	-	¾
Oxide of maganese		¼;

but it cannot be supposed that *proportions* deduced from experiments made on so small a scale can be entirely depended upon, although the *properties* of bodies may be so discerned, nearly as well as when larger quantities are employed.

An equal weight of tantalite taken from a specimen, of which the specific gravity of 7,8, yielded, by the same treatment,

White oxide - $4\frac{1}{2}$ grains
Oxide of iron - $\frac{1}{2}$
Oxide maganese $\frac{9}{10}$.

The white oxides obtained from each of these minerals, are remarkable for their insolubility in the three common mineral acids, as both Mr. Hatchett and M. Ekeberg have observed.

In muriatic acid they cannot be said to be absolutely insoluble; but they are not sufficiently soluble for the purposes of analysis.

In nitric acid they are also nearly, if not perfectly, insoluble.

In sulphuric acid, when concentrated and boiling, the oxide of columbium may be dissolved in small quantity, and so also may the oxide obtained from tantalite.

The proper solvent, as has been observed by Mr. Hatchett and by M. Ekeberg, is potash; and as it is not required to be in its caustic state, I employed the crystallized carbonate of potash on account of its purity and uniformity. Of this salt about eight grains seemed requisite to be fused with one of the oxide obtained from either of these minerals to render it soluble in water.

Soda also combines with the oxide, and may be said to dissolve it; but a far larger proportion of this alkali is necessary, and a larger quantity of water. And although a solution may have been effected that is transparent while hot, it very soon becomes opaque in cooling, and finally almost the whole of the oxide subsides combined with a portion of the soda in a state nearly insoluble.

When a solution of the white oxide, obtained from either of these minerals, has been made, as above, with potash, the whole may be precipitated by the addition of an acid, and will not be redissolved by an excess of sulphuric acid, of nitric, of muriatic, succinic, or acetic acids.

But there is a further agreement in the properties of these two minerals, which appears above all others to establish their identity; for though they are both so nearly insoluble by any excess of the mineral acids, yet they are each completely dissolved by oxalic acid, by tartaric acid, or by citric acid; and the solution of each is subject to the same limitations; for if the precipitate has been dried, it is become intractable, and can scarcely be dissolved again till after a second fusion with potash.

If to the alkaline solution of either of them there be added infusion of galls, prussiate of potash, or hydrosulphuret of potash, no precipitate occurs; but when a sufficient quantity of acid has been added to neutralize the redundant alkali, the infusion of galls will then occasion an orange precipitate; but prussiate of potash causes no precipitate, nor does the hydrosulphuret precipitate the oxide, although the solution may become turbid from precipitation of sulphur by a redundant acid.

The characteristic precipitant of columbium is consequently the infusion of galls; but in the employment of this test certain precautions are necessary. For as an excess of potash may prevent the appearance of this precipitate, so also may a small excess of oxalic or tartaric acids prevent precipitation, or dissolve a precipitate already formed. A larger excess of citric acid seemed requisite for that purpose, and would also dissolve the gallat of columbium. In each case the precipi-

tate may be made to appear by neutralizing the redundant acid; and for this purpose carbonate of ammonia should be employed: for although pure ammonia has no power of dissolving the oxide alone, yet the gallat seemed to be perfectly re-dissolved by that alkali.

When infusion of galls is poured upon the white oxide recently precipitated, and still moist, it combines readily and forms the orange-coloured compound.

Prussiate of potash occasioned no change in an oxide that had been purified by a second fusion with potash; but it appeared to dissolve a small portion of the oxide, as infusion of galls, poured into the clear liquor, occasioned a cloudy precipitate of an orange colour, though no such precipitate took place when the infusion was mixed with the same prussiate alone.

Hydrosulphuret of potash being added to the oxide, and heated upon it, impaired the whiteness of its appearance, and seemed to detect the remains of some impurity which had not yet been removed by other means; but no appearance indicated the formation of a sulphuret of columbium.

From a careful repetition of these experiments upon each of the oxides, I see no reason to doubt of their perfect agreement in all their chemical properties; but there is nevertheless a very remarkable difference in the specific gravities of the two minerals from which they are extracted.

The specific gravity of columbite was ascertained by Mr. Hatchett to be 5,918; that of tantalite was found by M. Ekeberg to be 7,953, and I have every reason to suppose their results correct, since a small fragment of the former appeared upon trial to be 5,87, while a specimen of tantalite,

weighed at the same time, was as much as 7,8. I should, however, observe, that the specific gravities of three other fragments borrowed for this purpose were not so high, that of one being 7,65, of another 7,53, and of a third so low as 7,15.

It is evident that no variation of mere proportion of the ingredients can account for an increase of specific gravity from 5,918 to 7,953, which are in the ratio of 3 to 4; for since columbite contains four fifths oxide, if the whole remaining one fifth part in weight of that oxide could be supposed added to the same bulk, without diminution of the quantities of iron and manganese, the specific gravity would not then exceed 7,1; and even if a weight equal to one third of the whole were thus added, without increase of bulk, still the aggregate would not quite equal the heaviest tantalite in specific gravity; but, on the contrary, the quantity of white oxide in this specimen certainly does not amount to six sevenths, and probably is not more than five sixths of the whole mass.

The only chemical difference, by which this circumstance could be explained, would be the state of oxidation, which my experiments cannot appreciate; but it may also arise in part from actual cavities in the mass of columbite, and in part from the state or mode of aggregation.

XVI. *Description of a reflective Goniometer.* By William Hyde Wollaston, *M. D. Sec. R. S.*

Read June 8, 1809.

From the advances that have been made of late years in crystallography, a very large proportion of mineral substances may now be recognised, if we can ascertain the angular dimensions of their external forms, or the relative position of those surfaces that are exposed by fracture. But though the modifications of tetrahedrons, of cubes, and of those other regular solids, to which the adventitious aid of geometry could be correctly applied, have been determined with the utmost precision, yet it has been often a subject of regret, that our intruments for measuring the angles of crystals are not possessed of equal accuracy, and that in applying the goniometer to small crystals, where the radius in contact with the surface is necessarily very short, the measures, even when taken with a steady hand, will often deviate too much from the truth to aid us in determining the species to which a substance belongs.

A means of remedying this defect has lately occurred to me, by which in most cases the inclination of surfaces may be measured as exactly as is wanted for common purposes, and when the surfaces are sufficiently smooth to reflect a distinct image of distant objects, the position of faces only $\frac{1}{15}$ of

an inch in breadth may be determined with as much precision as those of any larger crystals.

For this purpose, the ray of light reflected from the surface is employed as radius, instead of the surface itself, and accordingly for a radius of $\frac{1}{10}$ of an inch, we may substitute either the distance of the eye from the crystal, which would naturally be about twelve or fifteen inches; or for greater accuracy we may, by a second mode, substitute the distance of objects seen at a hundred or more yards from us.

The instrument which I use, consists of a circle graduated on its edge, and mounted on a horizontal axle, supported by an upright pillar (Plate XI). This axle being perforated, admits the passage of a smaller axle through it, to which any crystal of moderate size may be attached by a piece of wax, with its edge, or intersection of the surfaces, horizontal and parallel to the axis of motion.

This position of the crystal is first adjusted, so that by turning the smaller axle, each of the two surfaces, whose inclination is to be measured, will reflect the same light to the eye.

The circle is then set to *zero*, or 180°, by an index attached to the pillar that supports it.

The small axle is then turned till the further surface reflects the light of a candle, or other definite object to the eye; and, lastly, (the eye being kept steadily in the same place) the circle is turned by its larger axle, till the second surface reflects the same light. This second surface is thus ascertained to be in the same position as the former surface had been. The angle through which the circle has moved, is in fact the supplement to the inclination of the surfaces; but as

the graduations on its margin are numbered accordingly in an inverted order, the angle is correctly shewn by the index, without need of any computation.

It may here be observed, that it is by no means necessary to have a clean uniform fracture for this application of the instrument to the structure of laminated substances; for since all those small portions of a shattered surface, that are parallel to one another (though not in the same plane), glisten at once with the same light, the angle of an irregular fracture may be determined nearly as well, as when the reflecting fragments are actually in the same plane.

In this method of taking the measure of an angle, when the eye and candle are only ten or twelve inches distant, a small error may arise from parallax, if the intersection of the planes or edge of the crystal be not accurately in a line with the axis of motion;* but such an error may be rendered insensible, even in that mode of using the instrument, by due care in placing the crystal; and when the surfaces are sufficiently smooth to reflect a distinct image of objects, all error from the same source may be entirely obviated by another method of using it.

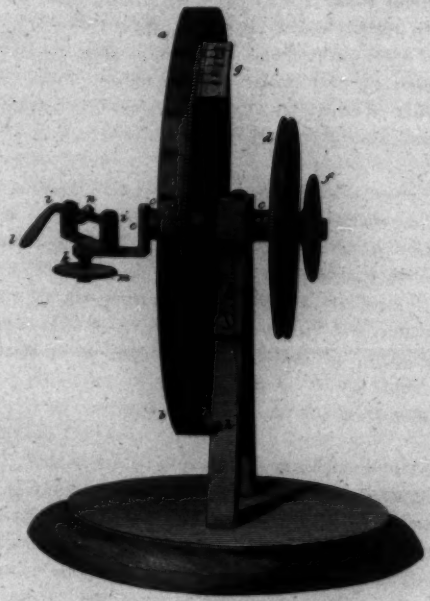
For this purpose, if the eye be brought within about an inch of the reflecting surface, the reflected image of some distant chimney may be seen inverted beneath its true place, and by

* I cannot omit mentioning, that Mr. Sowerby had thought of employing reflection for this purpose, nearly at the same time as myself; but did not succeed to his satisfaction, in consequence of an attempt to fix the position of the eye. For when the line of sight is determined by a point connected with the apparatus, the radius employed is thereby limited to the extent of the instrument, and the error from parallax is manifestly increased.

turning the small axle may be brought to correspond apparently with the bottom of the house (or with some other distant horizontal line.) In this position the surface accurately bisects the angle, which the height of that house subtends at the eye (or rather at the reflecting surface); then, by turning the whole circle and crystal together, the other surface, however small, may be brought exactly into the same position; and the angle of the surfaces may thus be measured, with a degree of precision which has not hitherto been expected in goniometry.

The accuracy, indeed, of this instrument is such, that a circle of moderate dimensions, with a vernier adapted to it, will probably afford corrections to many former observations. I have already remarked one instance of a mistake that prevails respecting the common carbonate of lime, and I am induced to mention it, because this substance is very likely to be employed as a test of the correctness of such a goniometer, by any one who is not convinced of its accuracy from a distinct conception of the principles of its construction.

The inclination of the surfaces of a primitive crystal of carbonate of lime is stated, with great appearance of precision, to be 104° 28′ 40″: a result deduced from the supposed position of its axis at an angle of 45° with each of the surfaces, and from other seducing circumstances of apparent harmony by simple ratios. But however strong the presumption might be that this angle, which by measurement approaches to 45°, is actually so, it must nevertheless be in fact about 45° 20′; for I find the inclination of the surfaces to each other is very nearly, if not accurately 105°, as it was formerly determined



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to be by HUYGENS;* and since the measure of the superficial angle given by Sir ISAAC NEWTON† corresponds with this determination of HUYGENS, his evidence may be considered as a further confirmation of the same result; for it may be presumed, that he would not adopt the measures of others, without a careful examination.

IN THE ANNEXED PLATE,

a b. Is the principal circle of the goniometer graduated on its edge.

c c. The axle of the circle.

d. A milled head by which the circle is turned.

e e. The small axle for turning the crystal, without moving the circle.

f. A milled head on the small axle.

g. A brass plate supported by the pillar, and graduated as a vernier to every five minutes.

h. The extremity of a small spring, by which the circle is stopped at 180°, without the trouble of reading off.

i i and *k k.* Are two centers of motion, the one horizontal, the other vertical for adjusting the position of a crystal: one turned by the handle *l*, the other by the milled head *m*.

The crystal being attached to a screw-head at the point *n* (in the center of all the motions), with one of its surfaces as

* HUYGENII Opera Reliqua, Tom. I. p. 73.—Tract. de Lumine.

† NEWTON's Optics, 8vo. p. 329. Qu. 25, concerning Iceland Crystal.

nearly parallel as may be to the milled head *m*, is next rendered truly parallel to the axis by turning the handle *l* till the reflected image of a horizontal line is seen to be horizontal.

By means of the milled head *f*, the second surface is then brought into the position of the first, and if the reflected image from this surface is found not to be horizontal, it is rendered so by turning the milled head *m*, and since this motion is parallel to the first surface, it does not derange the preceding adjustment.

XVII. *Continuation of Experiments for investigating the Cause of coloured concentric Rings, and other Appearances of a similar Nature.* By William Herschel, *LL. D. F. R. S.*

Read March 23, 1809.

In the first part of this paper, I have pointed out a variety of methods that will give us coloured concentric rings between two glasses of a proper figure applied to each other, and it has been proved that only two surfaces, namely, those that are in contact with each other, are essential to their formation; it will now be necessary to enlarge the field of prismatic phenomena, by showing that their appearance in the shape of rings has been owing to our having only used spherical curves to produce them.

35. *Cylindrical Curves produce Streaks.*

As soon as it occurred to me, that the cause of the figure of any certain prismatic appearance must be looked for in the nature of the curvature of one or both of the surfaces, that are essential to its production, I was prepared to expect that if a spherical curve, when applied to a plain surface of glass, produces coloured rings, a cylindrical one applied to the same would give coloured lines or streaks. To put this to the proof of an experiment, I ground one side of a plate of glass into a cylindrical curve, and after having given it a polish, I laid a slip of plain glass upon it, and soon perceived a beautiful set of

coloured streaks. The broadest of them was at the line of contact, and on each side they were gradually narrower and less bright. The colours in the streaks were similar to those in the rings, and they were in the same manner changeable by pressure as in them. Their order was likewise the same, if we reckon from the line of contact, as with rings we do from the center; so that these streaks differed in no respect from rings, except in their linear instead of circular arrangement.

When the cylindrical surface was laid upon a plain slip of glass, the same streaks were seen as in the former experiment. They were of a lively red and green colour, and I saw at least ten, eleven, or twelve on each side of the line of contact.

Metalline surfaces had the same effect, for when the cylindrical surface of glass was laid on a plain metalline mirror, I had red, orange, yellow, green, and blue streaks. In the same manner a plain slip of glass placed upon a polished part of a brass cylinder of $3\frac{1}{2}$-inch in diameter, produced also coloured streaks.

The combination of two cylindrical surfaces has an effect on the streaks, which is similar to that which the contact of two spherical ones has on the rings; for when I placed the cylindrical surface of glass longitudinally upon the polished part of the brass cylinder, the streaks were contracted as rings would have been by the application of two spherical curves to each other.

36. *Cylindrical and spherical Surfaces combined produce coloured elliptical Rings.*

The theory which suggests to us that the particular figure of every prismatic appearance between glasses depends on the curvature of the surfaces which are in contact, is still farther confirmed when spherical and cylindrical curves are applied to each other; for these, accordingly, should give elliptical rings; and when I tried the experiment, by laying a 16-inch double convex lens upon the cylindrical surface of my plate of glass, it produced a coloured elliptical central part, encompassed with gradually vanishing rings of the same figure. By changing the focal length of the lens, I could alter the proportion of the conjugate to the transverse axes of these elliptical rings at pleasure. A lens of 55 inches gave ellipses that were much flattened, and one of 5 inches gave them nearly circular.

37. *Irregular Curves produce irregular Figures.*

The modifying power of surfaces may be further established by such as have no regular figure; for these ought to give irregular prismatic phenomena, and this was fully proved by the following experiment.

I took a large piece of mica which had a very glossy but irregular surface, and when a 34-inch double convex lens was placed upon a small ridge of it, several pretty straight streaks might be seen, but wherever the ridge was waving the streaks were following the same direction. In some places the mica gave irregular, coloured arcs, that were concave to some distant centre; and in others, the various contorted figures, that

were to be seen, exceeded all the imaginary forms which the most inventive fancy can paint. The flexibility of mica also gave room for using different degrees of pressure, by which means a continual change of figure and succession of prismatic colours was produced.

When I laid a piece of this mica upon a cylinder, and placed a plain slip of glass or double convex lens upon it, all its irregularities were modified into disfigured streaks with the former, and distorted ellipses with the latter.

Experiments of a similar nature were made upon the irregular surface of Island crystal and other substances, which all gave the same result.

38. *Curved Surfaces are required for producing the coloured Appearances at present under Consideration.*

It has already been seen, in the first part of this paper, that spherical curves give circular rings, and I have now shown that cylindrical forms produce streaks; that a combination of spherical and cylindrical curvatures give elliptical rings, and that all sorts of variegated coloured phenomena are made visible by surfaces, which are irregularly and variously curved; these experiments prove in the fullest manner that the curvature of surfaces is the cause of the appearance, as well as of the shape of the coloured phenomena which are produced. For if we can invariably predict, from the nature of the curves we employ in an experiment, what will be the appearance and form of the colours that will be seen, it certainly must prove the efficacy of these curvatures in the production of such phenomena. This will receive additional confirmation in the following article, which shows that

39. *Coloured Appearances cannot be produced between the plain Surfaces of two parallel Pieces of Glass applied to one another.*

As the production and modification of the figure of the coloured appearances, that have hitherto been considered, has in the last article been ascribed to curved surfaces, it will be necessary to examine whether such phenomena may not also be seen between the plain surfaces of two parallel pieces of glass applied to each other directly in contact, or inclined towards each other in some certain extremely small angle.

The latter of these cases has already been considered in the 31st article of the first part of this paper, where I have shown that two plain surfaces, let the angle of the wedge of air between them be as small as you please, will not give coloured streaks. I have indeed seen two thin plain pieces of glass, with a slip of platina of an extraordinary thinness between them at one end tied together, which showed some streaks near the place where the glasses were in contact, but when I removed the thread that bound them together, the streaks vanished, which proves that the glasses had been constrained, and thus had probably assumed some curvature at the point of contact.

I have also tried two flat surfaces of glass, which were so perfect that no colour could be perceived unless they were by unequal pressure somewhat disfigured, and when that was the case large flashy coloured appearances became visible, and their configuration followed very evidently the stress which I laid upon the different parts of the glasses.

It is however unnecessary to dwell on proofs, that streaks cannot be seen when two plain parallel pieces of glass are

applied to each other, as it will hereafter be shown that when the incumbent plain glass is not of a parallel thickness, coloured phenomena may be rendered visible between two perfectly plain surfaces, although no force or strain should be used to produce a fallacious, curved, contact.

40. *Of the Production of coloured Appearances.*

Hitherto I have only considered the coloured rings which Sir ISAAC NEWTON has pointed out, and have shown, at the end of the 28th article, that no more than two surfaces are essential to their formation. It has now also been proved, that the configuration of the coloured phenomena arises from the curvature of one or both of the two essential surfaces. From these principles it will be seen, that we are to distinguish between the production of the colours and that of their configuration when produced. By the experiments that have been given, the cause of the configuration is laid open to our view; but the production and arrangement of the colours remain to be investigated.

The leading feature of the arrangement of the colours of the rings is prismatic; that is to say their order is red, orange, yellow, green, blue, indigo, and violet; in order, therefore, to enter minutely into the subject, I shall have recourse to some prismatic experiments.

It will be necessary here to mention, that the proposed enumeration of the modifications of light, which was intended to have been given in this part of my paper, is grown to such an extent by the number of experiments I have made upon the subject, that its introduction would occasion a long interruption of the present subject; and although undoubtedly the

action of bodies and surfaces on light would be better understood, if all the modifications wherein colours are produced had been before us, yet as the experiments I have to relate may be made plain, either by referring to modifications that are sufficiently known, or by explaining what is not already familiar, I shall postpone the intended enumeration to some future opportunity, and confine myself at present to a few remarks relating to them.

The colours contained in white light may be separated by reflection, as well as by refraction, and what is perfectly to my present purpose, the order, in which the colours thus produced are arranged, is the same in both cases; each of these principles therefore may cause coloured appearances, which the particular figure of the surfaces we use will mould into different configurations.

Sir ISAAC NEWTON, for instance, has shown that the rays of light will be separated, by what he calls a different reflexibility, when they fall on the base of a prism; the violet being reflected first, and the red last.* By this property of the differently coloured rays, he has explained a very remarkable phenomenon, which is that in a prism, when exposed in the open air, and when the eye is properly placed "the spectator will see a bow of a blue colour."† From the little the author has said of this bow, it may be supposed that he did not examine it farther than was required for his purpose; it will therefore be necessary to enter more fully into the subject.

* See the illustration of the 9th experiment in the first book of NEWTON's Optics, page 46.

† See the 16th experiment in the second part of the first book, page 145.

41. *Particulars relating to the Newtonian prismatic blue Bow.*

The Newtonian blue bow may very conveniently be examined, when a right angled prism is laid down on a table before an open window. The eye being then brought to a convenient altitude, and pretty near the side of the prism, we see in it a bow, which from the predominant colour may be called blue. It contains some green followed by blue, indigo, and violet. A very faint red, orange, and yellow may also be perceived above the greenish colour; but these belong not to the blue bow, and have not been noticed by the author. Their appearance will hereafter be accounted for.

To analyse this blue bow more particularly, let us admit that the colours, which give it the general appearance of what may be called blue, consists of half the green, and of all the blue, indigo, and violet rays, which are reflected while the other half of the green, the yellow, orange, and red are transmitted. Then the angle of obliquity, at which this separation of the colours will happen, in consequence of the different refrangibility of the differently coloured rays assigned by NEWTON, will be 49° 46′ 12″,5.

Let ABCDE, Plate XII. Fig. 1, be rays of light moving within glass in such directions, as to fall on the interior base FG upon the points $\alpha\,\beta\,\gamma\,\delta\,\epsilon$. Then, if it be required that these rays after reflection from the base should meet in the point H, and form the blue bow, the angles $A\alpha G$, $B\beta G$, $C\gamma G$, $D\delta G$, and $E\epsilon G$ must be respectively equal to 49° 46′ 12″,5; 49° 49′ 20″; 49° 55′ 33″,6; 49° 59′ 41″,4; and 50° 7′ 54″; which will give the angles $\alpha H\beta$, $\beta H\gamma$, $\gamma H\delta$, $\delta H\epsilon$, equal to 3′ 7″,5, 6′ 13″,6, 4′ 7″,8, and 8′ 12″,6, making in the whole

the angle subtended by the bow $\alpha H \beta$ 2′ 41″,5.* For in consequence of the different reflexibility of the differently coloured rays the violet, indigo, blue, and faintest half of the green rays will be reflected between α and β, if they fall on that space in any angle between the above mentioned ones contained between $A \alpha G$ and $B \beta G$; and will therefore meet at H, and form the greenish blue part of the bow. The red, orange, yellow, and the brightest half of the green rays, on the contrary being less reflexible, will be transmitted through the base between α and β, and by refraction pass in proper angles into the air. The letters $vib\frac{1}{28}g$, which in the figure are placed within the space $\alpha H \beta$, denote the reflected colours, and $\frac{1}{2}g\,y\,o\,r$ put under the base between α and β are the initials of the transmitted colours; and in the same manner the reflections and transmissions which must happen between $\beta\gamma$, $\gamma\delta$, and $\delta\varepsilon$ are expressed by the letters over the base for the former, and under it for the latter. The order of the colours of the blue bow, when it is seen at H, is perfectly explained by the letters in the reflected part; and the eye must be placed, for seeing it, at the mean obliquity between the angles $A \alpha G$ and $E \varepsilon G$, which is 49° 57′ 8″,3.

In order to conform this account of the blue bow, to the manner in which it was viewed by Newton, I have preserved his way of ascribing the separation of the rays to their different reflexibility, which however is merely the effect of their

* There is a mistake in one of the angles given by Newton, when in his Optics, page 145, he explains the blue bow: for 49 deg. $\frac{3}{11}$ taken from 50 deg. $\frac{1}{4}$, makes the breadth of the bow 1° 4′ 31″,4, which contradicts the refractions he has given, page 112. As he only takes in the blue, indigo, and violet colours, instead of 49$\frac{3}{11}$ degrees, it should rather be 49$\frac{1}{11}$.

different refrangibility. The angles at which the rays that constitute the blue bow are separated from the rest, may very properly be called *critical*, and the effect, which is the consequence of the oblique incidences that have been given, may with equal propriety be called a *critical separation* of the differently coloured rays of light.

42. *Account of a prismatic red Bow.*

I must now introduce a prismatic appearance, which on account of its similarity with the Newtonian blue bow, from which it only differs in colour, I have called, a prismatic red bow. It consists of red, orange, yellow, and some green rays; and the red colour being upon the whole very predominant, it may not improperly be called a red bow. It is not produced by the Newtonian different reflexibility of the differently coloured rays of light, but owes its origin to a modification which takes effect at the outside of the prism at very oblique angles of incidence, and may be called a different intromissibility; but this, like the Newtonian different reflexibility, is only the consequence of the different refrangibility of light.

To see the red bow, an observer should place himself in the open air, and standing with his back within a few feet of some wall or building, hold the side of an equilateral prism flat over his eyes, and look upwards to an altitude of about 80° at the heavens; he will then see a beautiful arch of a deep red colour, succeeded by a bright orange and yellow, with a considerable portion of green on the inside. The comparative darkness of the building behind will show the light in front to the best advantage. It is also to be observed, that all

experiments on prismatic bows succeed best, when the heavens are totally overcast with an uniform cloudiness.

To analyze the production of this bow, let ABCDE, Fig. 2, Plate XII. be rays of light moving in air, in such directions as to fall on the exterior base FG, of a piece of glass, upon the points $\alpha\ \beta\ \gamma\ \delta\ \epsilon$; then, if it be required that these rays, after their intromission into the glass should meet in the point H and form the red bow, the angles $A\alpha H$, $B\beta H$, $C\gamma H$, $D\delta H$, and $E\epsilon H$, must be respectively equal to 130° 29′ 33″,6; 133° 40′ 33″,2; 134° 29′ 28″,2; 135° 36′ 13″,2; and 136° 10′ 38″,0; from which we have the angles $A\beta B$, $A\gamma C$, $A\delta D$, and $A\epsilon E$, which a red ray would make were it to pass out of glass into air, equal to 3° 15′ 45″,5; 4° 7′ 30″,5; 5° 19′ 17″,5; and 5° 56′ 50″,5. Now by the laws of the different refrangibility of light, the red rays are intromissible at α, when by refraction they make the angle $H\alpha F = 49° 30′ 26″,4$; but the orange cannot be intromitted any where between α and β with any effect on the red bow, since it is only at β, where the angle $H\beta F$ is 49° 35′ 12″,8, that they can enter the glass so as to come to the eye at H. The yellow rays will, for the same reason, be efficiently intromitted only at γ, where they will make the angle $H\gamma F$ 49° 38′ 2″,8, and the brightest half of the green rays will find an efficient entrance from δ to ϵ, since the smallest angle of their intromission $H\delta F$ is 49° 48′ 4″,8, and the angle $H\epsilon F$, which terminates the red bow, is 49° 46′ 12″,5. The arrangement of the colours of this bow will be seen, as it was in the blue bow, from the letters placed above the base, which denote those that are intromitted so as to come to the eye; the rest of the colour-making rays, which cannot come in that direction, being marked by letters placed under the base. The

whole angle of the red bow α H α is 15′ 46″,1, and the mean obliquity of the eye at H is 49° 38′ 19″,5.

In the calculation of both the bows, the situation of the eye at H has been determined, as it would be, were the rays to remain in glass; but as they will be refracted by the side of a prism, when they come out of it, proper computations must be made not only of the place of the eye in air, but also of the angle which the bow will subtend; for this will be found to be considerably different in different prisms; those that have large refracting angles will magnify the bows more, and require the eye to be nearer than others that have smaller angles.

These bows may be examined at leisure, by projecting them upon a white ground in the following manner:

In a dark room, by a reflecting apparatus, I admitted a horizontal beam of the solar light through an opening of about an inch and a half in diameter. The formation of the bows requiring scattered light,* I covered the opening with a piece of glass evenly roughned on both sides. Then, with an intention to obtain a projection of the blue bow, I placed a prism having one angle of 91° and the other two nearly equal, close to the emeried surface, and turned it upon its axis till the angle of obliquity of the scattered rays, that fell on one side of the prism, was proper for the required critical separation of the coloured rays. The obliquity of the middle ray with the base, for this purpose, it has been shown, must be 49° 57′ 3″,3. In this position the interior critical separation of the prismatic colours taking place, the blue part, namely the violet, indigo, blue, and about one half of the green rays were

* See the first paragraph of the 46th article of this paper.

reflected, and passing through the opposite side of the prism projected the blue bow upon the cieling of the room. The colours may there be conveniently seen; but as this bow is composed of the least luminous rays of the prismatic spectrum, it requires considerable attention to perceive the faintest of them. The green and blue are most visible, and by receiving the bow upon a screen of white paper held at the most favourable distance, the fainter colours, when the illumination is very bright may also be perceived.

In order then to project also the red bow, I turned the prism upon its axis till the scattered light fell with a proper obliquity on the base of it; the angle required for this purpose, it has been shown, must be from $0^{\circ}\ 0'\ 0''$ to $5^{\circ}\ 56'\ 50''5$; the side of the prism, which is turned towards the opening, should be covered with a slip of pasteboard to prevent any light from entering it. In this situation, I saw a very bright arch containing red, orange, and yellow projected at some distance backwards upon the cieling; that part of the green which no doubt was also transmitted, was lost in the brightness which is to be seen within the bow, for the same reason that the faint colours of the blue bow can only with great difficulty, if at all, be perceived; namely, that they join the dark inside of the bow. For Newton has proved that the space beyond the convex part of the blue bow must be bright, and that beyond the concave dark; but in the red bow, as my theory will show, we have that on the convex dark, and on the concave bright. This experiment therefore proves, that here, by the gradual intromission of the differently coloured rays, a critical separation takes place on the outside of the prism, similar to that which by reflection happens in the blue bow at the in-

side; and by which, in the present case, the red part of the prismatic spectrum, that is, the red, orange, yellow, and some of the green, can only reach the eye.

43. *Of a sudden Change of the Colours of the Bows.*

It has been shown that the red bow should be seen nearly in the same place where the Newtonian blue bow is visible. For in the 41st article the place of the eye, for seeing the blue bow in the prism of 100 degrees, was determined to be at an obliquity of 49° 57′ 3″.3; and with the red bow, and in the same prism, it has been shown that the eye must be placed at the obliquity of 49° 38′ 19″.5. The difference is only 18′ 43″.8, and by the following experiments it will be found, that both the bows may actually be seen nearly in the same part of every prism; and that the direction of the light, by which we see either the blue or the red bow, determines which of the two will be visible. To prove this, let a right angled prism be laid down on a sheet of white paper before a window, and when the eye is placed in the proper situation for seeing a reflected blue bow, we may instantly transform it into a transmitted red one, by covering the side of the prism which is towards the incident light with a slip of pasteboard; for by stopping the direct light, which before fell on the base of the prism, and was there reflected, we then see the bow by light intromitted from the paper through the base, which, as has been explained, will be red.

With proper management we may have the bow half red and half blue; blue in the middle with red sides, or red in the middle with blue sides; which appearances it will not be required to explain any farther, especially after what has

already been said in the 18th article of the first part of this paper of the change of the colours of rings.

When we have before us a bow that is half blue and half red, it will be seen that both taken together contain all the prismatic colours in their regular order of refrangibility. It will now also appear that the faint red, orange, and yellow, which I have said are to be perceived above the blue bow* may arise either from an imperfectly transmitted red bow, which always lies concealed under the Newtonian blue one, or perhaps more probably from the partial reflection of the red, orange, and yellow rays, many of which will come to the eye notwithstanding they are also copiously transmitted.†

According to my account of the red bow, it ought to be seen in the prism a little above the blue one, and this is also further confirmed by any one of the experiments in which we have some part of each bow in view at the same time, for then the relative situation of the two bows will be visible.

Similar experiments may be made by candle light upon either of the bows; for when a sheet of white paper is pinned against a wall, that it may reflect the light of a candle placed upon a table about three or four inches from the paper, we may then see the blue bow in a prism placed upon a dark ground before the reflecting paper; and the green colour, which it is not very easy to perceive distinctly in daylight, will here be very visible, and the more so if we use an equilateral

* See the first paragraph of the 41st article.

† In my modifications of light I have proved, by undeniable experiments, that within a prism as well as on the outside of it the rays of all the colours are equally reflexible, and that a critical separation of them only takes place at those angles where by refraction a ray cannot be transmitted.

prism instead of a right angled one. When the reflecting paper is removed from the wall and laid under the prism, that the light may then be thrown upwards and transmitted through the base, we see a bow of a lively red colour.

Before I can introduce more intricate phenomena, it will be necessary to advert to some other particulars relating to these bows.

44. *Of Streaks and other Phenomena produced from the prismatic blue and red Bows.*

It has been remarked in the 40th article, that the production of colours and their configuration when produced are owing to different causes; this will now be confirmed by an experiment.

Scattered rays, when they fall on a prism will by a critical separation of the colours, produce both the blue and the red bows, and these coloured appearances when produced may be modified into streaks, circular rings, and other forms, by the configurating power of surfaces. When a plain glass or metalline mirror is laid under the base of a right angled prism in which we see the blue bow, the contact of the two plain surfaces will immediately produce a great number of coloured streaks. They will be found to be parallel to the bow, most of them within and some just under it. They may be seen without any lens, merely by looking into the prism with the eye pretty close to the surface through which we see the blue bow. This experiment proves that plain surfaces, though they cannot produce colours, have a power of modifying and multiplying them when produced. As I shall have occasion hereafter to be more particular, I shall now only mention that when we lay a

spherical surface, such as an object glass, under the prism it will immediately give us several sets of innumerable concentric coloured rings; and, as will now be readily expected, a cylindrical surface placed under the prism will give a number of lenticular appearances, such as are contained between the intersections of two circular arches drawn concave towards each other. The irregular surface of mica will in like manner produce multiplications of appearances, that may be seen much better than they can be described.

When the same surfaces are applied to the red bow, phenomena that are perfectly of the same form will be made visible within and just under the bow; and the streaks will also be in a parallel direction.

The side of the prism, to which a plain glass must be applied, is of singular use in the explanation of many appearances of the coloured phenomena, which are to be seen, and it is on this account that the formation of the generated colours into all sorts of configurations has been noticed before I come to that part of this paper, wherein this subject must find a further discussion; for by the application of a slip of plain glass, we can decisively ascertain the nature of any coloured appearance in the prism. Thus, when we see a common coloured red or blue arch, occasioned by the mere different refrangibility of light, the plain glass any how applied to the prism will give no streaks. If we apply the plain glass to a transmitting side, we can have no streaks from a critical blue bow, because it is occasioned by reflection; and for the same reason, when the plain glass is applied to a reflecting side, we can have no streaks that belong to a critical red bow, because it originates at the intromitting surface. With the assistance of

this criterion I may now proceed to a review of more complicated phenomena.

45. *Explanation of various Appearances relating to prismatic Bows.*

If, in the open air, we look into the zenith with a right angled prism held across the eyes, we shall see two red bows convex towards each other. They are caused by the bright transmissions of the light of the heavens through the sides in which the bows appear; for when to either of these sides the criterion of the plain glass is applied, we shall have coloured streaks. The course of the rays which produce the two bows is delineated in Fig. 3, Plate XII. ABC represents the prism, and the rays that can enter the eye when they fall on AB within the limits *a b* A from 0° 0′ 0″ to 5° 56′ 50″,5, which are the red, orange, yellow, and the brightest part of the green, will form the red bow; and the situation of the eye at E will be had by the mean refrangibility of the rays which give the bow; for as the angle B*c d* must be 49° 38′ 19″,5, we have the obliquity B*d c* = 85° 21′ 40″,5 and the angle C*e*E that conveys the ray to the eye will be 82° 49′ 34″,2. The same thing will happen on the other side of the prism, where the rays *m n o p q* will come to the eye at E, in an equal but differently directed angle B*q* E, and cause an inverted red bow to be seen in the side AC.

When we look down into the side of an equilateral prism we see a blue bow, but on lifting the eye and prism gently up together towards the zenith, the bow, at a certain altitude, will be changed from blue to red; and by the application of the criterion, it is proved that we see the first by reflection,

and the last by transmission. For, suppose *ab*, Fig. 4. Plate XII. to be a ray of a mean refrangibility between the violet, indigo, blue, and half the green; when this falls on the side A C of the equilateral prism A B C with an obliquity *ab* A of 57° 58′ 28″,5, it will be refracted so as to make the angle C *cd* 70° 2′ 56″,8 which gives 49° 57′ 8″,3 for the angle C *dc*; and consequently the ray *def* E will come to the eye by the same angles of reflection and refraction as it entered the prism, and make A *f* E equal to A *ba*. The eye at E will therefore see a blue bow. Then if a plain glass be applied to the transmitting side A C there can be no streaks; for blue bows being caused by the critical separation of the rays occasioned by the Newtonian reflexibility, the plain glass must be in contact with the reflecting side; and as soon as we hold it against B C, the coloured streaks will make their appearance. The change of the colour of the bow, on lifting the prism and eye together towards the zenith, is represented in figure 5; for the light from the sky, which will enter the prism on the side A B, will eclipse the blue bow which was seen before by light entering from the ground through the side A C in figure 4; then if *ab* fig. 5 is a ray of the mean refrangibility of the red bow, it will by refraction give the angle B *cd* 49° 38′ 19″,5, from which we obtain B *db* equal to 70° 21′ 40″,5, and the ray will, by a second refraction, come to the eye in an angle C *e* E of 38° 44′ 12″,4, where the red bow will be seen; but in order to produce coloured streaks, the plain glass must now be applied to the transmitting side A B.

When a right angled prism is held in the hand, so that the light of the sky through an open window may fall upon the base, if then an observer with his back to the light looks through

the base into the side AC of the prism ABC fig. 6, he will see an erect blue bow by two reflections, only one of which however is the cause of the critical separation of the coloured rays, the other being a common one. For when a mean refrangible blue-bow-ray falls with an obliquity abC of 82° 17′ 31″ on BC, it will by refraction give the angle $Bcd = 85° 2′ 56″,8$, from which we obtain $Ade = 49° 57′ 5″,8$, which being the mean angle of the critically separated rays, they will by reflection pass to the side AC, where the angle of the common reflection Cef will be 40° 2′ 56″,8; this gives efB 85° 2′ 56″,8, and by refraction the middle of the blue bow will be seen by an eye at E in an angle Egb equal to the angle abC. From the construction of the figure, it is evident that the eye may be drawn from E towards *a*, and always keep the blue bow in view, which will still remain erect; for when the eye comes to *a*, the rays by which the bow is seen will then enter at E, and the critical reflection will still remain at *d*, as may be satisfactorily proved by an application of the plain glass to AC, which will cause no streaks, whereas they will immediately appear when it is held under the side AB.

When the eye looks into the side BC with the same obliquity of 82° 17′ 31″, but differently directed, so that in fig. 7 the angle may be abB, instead of abC a blue bow will again be seen, but in an inverted position. This also may be drawn over into the other side of the prism without an alteration of its appearance, the reason of which is sufficiently evident from the construction of the figure; but in this case the critical reflection will be a *e*, and the common one at *d*.

It will be proper to shew that like appearances of the red bow may be seen; for this purpose let the prism be laid with

one side upon a sheet of white paper placed in a window, with the base towards the observer, as represented in fig. 8. In this position, the light from without reflected by the paper under the prism will be brighter than that from within the room, and the very oblique incident rays *a b* will be refracted by the horizontal side A B, so as to make the angle B *c d* equal to 49° 38′ 19″,5, from which we have B *d c* = 85° 21′ 40″,5, and by refraction C *e* E = 82° 49′ 34″,2, the eye placed at E will therefore see an erect red bow in the horizontal side A B, which may be drawn over into the perpendicular side without change of position; for the scattered rays reflected from the paper will also enter the prism in the same oblique angle of incidence from the opposite direction *a b* fig. 9; where having caused the red bow by an intromissive critical separation at *c*, they will come to the eye after a common reflection from the side A C, in the same angle as before.

When an inverted red bow is to be seen the eye must be placed a little lower, and the calculation of the angles in the 10th and 11th figures, which represent the course of the rays, being similar though differently directed, will be sufficiently understood by an inspection of them; but as in fig. 8 and 9, the intromissive separation was produced by the horizontal side, so it is, in these figures, effected by the vertical one; all which may be proved by a proper application of the criterion.

There are many other phenomena attending the bows, but as they are more intricate, and not necessary for my present purpose, I leave them to the ingenuity of those who have entered into the preceding calculations, which are quite sufficient to point out the method that should be taken for explaining them.

46. The first Surface of a Prism is not concerned in the Formation of the blue Bow, nor of the Streaks that are produced by a plain Glass applied to the efficient Surface.

It has already been mentioned that the bows are formed by scattered light; but to have a direct experimental proof that such light, if not absolutely necessary to the formation of the bows, is at least equally efficient with regularly refracted light, I took a prism with one side of it roughened on emery, and receiving the light through it when the eye was in the situation required for seeing the blue bow, I saw it as completely formed by scattered light, as it could have been by light regularly refracted through a polished side.

A natural consequence of this experiment seems to be, that the form of the surface through which light enters can be of no consequence; this will however admit of a more convincing proof, as follows: upon the middle of the side of a right angled prism, through which the rays entered that caused the blue bow, I laid a plano convex lens of an inch and a half focus; the result was, that not the least alteration could be perceived either in the form or in the colour of the bow, both which remained as perfect under the place where the incident rays passed through the lens as they were on each side of it. When I changed the convex lens for a plano-concave glass of the same focus, appearances were still the same; and when by a critical application of a plain glass I produced coloured streaks from the base of the prism, the interposition of either the convex or concave glass was equally immaterial. A scattering glass applied to the incident ray, had no other effect than to diminish the brightness of the bow.

The same experiment may be repeated with the red bow; but as here the first surface is essential to the formation of the bow, the plain side of the convex lens or concave glass, when placed against the prism, as before, will produce streaks; neither the bow, nor its streaks however will be in the least affected by the convexity or concavity of the outward surface of the glass applied, through which the light is admitted. A scattering glass will have no effect to disturb the bow or its streaks, and when this glass is emeried on both sides, we have again the bow complete, but without streaks; and by this fact it is proved, that unless a polished plain reflecting surface is applied to the prism, streaks cannot be formed.

47. *The Streaks which may be seen in the blue Bow contain the Colours of both the Parts of the prismatic Spectrum, by the critical separation of which the Bow is formed.*

The most favourable way of observing the colours of the blue bow streaks that are formed when a plain glass is laid under the base of a right angled prism, is to place a screen of white paper, before an open window, and to let the direct solar light shine through it upon the side of the prism. This scattered light will be bright and uniform, and cause no adventitious colours to mix with the streaks. The eye should be within six or seven inches of the prism. A streak consists of a certain principal colour and the intermediate tint which separates it from the next; and in the following memorandum of fourteen streaks, which I saw in the manner above described, the principal colours are placed in front, and the dividing tints at the side between them.

1. Very faint blue,
- - - - - - Pale red.
2. Faint blue,
- - - - - - - Pale red.
3. Blue,
- - - - - - - Pale red.
4. Bright blue,
- - - - - - - Faint red.
5. Purple blue,
- - - - - - - Whitish red.
6. Bluish red,
- - - - - - - Whitish red.
7. Deep red,
- - - - - - - - Greenish white.
8. Red,
- - - - - - - - Greenish white.
9. Red,
- - - - - - - - Pale bluish green.
10. Red,
- - - - - - - - Pale bluish green.
11. Pale red,
- - - - - - - Pale bluish green.
12. Paler red,
- - - - - - - Dirty white.
13. Dingy yellow,
- - - - - - Dirty white.
14. Dingy yellow.

To ascertain whether the second surface of the subjacent glass, which by other experiments I know to have a multiply-

ing power of at least six or seven reiterated interior reflections, all of which may be seen through the side of the prism, had any share in the production of these streaks, I fixed on one side of it a glass, of which the lowest surface was emeried, and on the other a metalline plain mirror, but found that the streaks were both in number and colour perfectly alike in them all.

By this account it is evident that the streaks derived from the blue bow contain not only the colours of the blue reflected, but also those of the red transmitted part of the spectrum. This fact is a clear indication of the office which is performed by the surface of the subjacent plain glass, which is simply that of reflecting back the rays of the transmitted red part of the spectrum, which being mixed with the blue part, both together, by their intersections, produce the observed streaks, as will be explained hereafter.

That the colours of the transmitted part of the spectrum are reflected back into the prism, is a point which I suppose will be admitted; but if it should be imagined that the red rays in the streaks of the blue bow might come into the prism by a scattered reflection of the light which falls on the plain glass under its base, then I say that a sheet of white paper or double emeried glass, ought to give the brightest streaks; whereas, on the contrary, neither of them produces any;* it is therefore evident, that a regular reflecting surface is necessary to their formation; but such a surface, be it glass or metal, can only reflect red rays when it receives them; and since we know that the red part of the spectrum is transmitted, and must fall on the reflecting surface, it is but fair to conclude that the

* See the last paragraph of the preceding article.

rays, of which that part is composed, are those which by reflection re-enter the prism.

48. *On the Formation of Streaks.*

As I have now ascertained that the streaks we see when a plain glass is laid under a prism, which shows the blue bow, are formed by the principle of reflection, which throws back the transmitted rays, it will be a considerable satisfaction if we can trace the course of these rays far enough to have some idea of the arrangement, whereby such appearances may be produced. To show, by calculation, the complete formation of the streaks in a case that is liable to such variation, on account of the different contact between the modifying surfaces, the position of the light and the inclination of the eye, would be a most laborious, if not endless, undertaking; it will therefore be sufficient, if I can make it appear, that streaks must unavoidably be produced by the rays which after transmission are reflected back again, and mix with those that form the bow; and this I believe will not be difficult. For instance, let F G, fig. 12, Plate XIII. be the base of a solid piece of glass, in which a compound ray of light is moving from A to α, with an obliquity $A\alpha G = 49^\circ\ 46'\ 12,''5$; and let I K be the plain surface of a reflecting substance placed under the base; then will the violet, indigo, blue, and the faintest part of the green of this ray be reflected at α, and the remaining green, the yellow, orange, and red will be transmitted. Now, in order to understand the intention of this figure, it will be necessary to observe that on account of the minuteness of the operations of light, all the lines and distances are represented upon a scale one thousand times larger than what the calculation gives

them. The real dimensions of several lines therefore cannot find room in the figure, and must be supplied by imagination. The distance of the eye from the base F G, for instance, which in the calculation has been assumed to be only three inches, will be 3000; the diameter of the pupil of the eye 200; the breadth of the base not less than 2160; and the subtense of the whole blue bow will be twenty-four inches eight tenths. The distance between the reflecting surface I K, and base F G, I have supposed to be the ten thousandth part of an inch; it is therefore in this figure represented by one tenth of an inch, and the space $\alpha\beta$, in which the colours that have been mentioned are transmitted, and which by calculation is ,003588 is expressed by 3,59 inches.

The rays of the different colours which are transmitted at α will be refracted in different angles, and when they come to the reflecting plane will be returned to the base in such a direction, as to come to it again in the same angle in which by refraction they left it; but their distance from the point α, when they reach the base, will differ considerably. If we call the angle of refraction φ, and the distance of the reflecting plane from the base x, then $2x \times \frac{\text{rad.}}{\text{tan. } \varphi}$ will be an expression for the intervals at which the several rays will re-enter the base, which for red will be $\alpha r =$,0019198, for orange $\alpha o =$,0022974, for yellow $\alpha y =$,0026675, and for green $\alpha g =$,0043053. At these places the rays will be a second time refracted, and rise towards the eye in parallel directions, and with an obliquity of 49° 46′ 12″,5 equal to that of their incidence A α G. Their course is represented in the figure by the letters $\alpha r r' r''$, $\alpha o o' o''$, $\alpha y y' y''$, and $\alpha g g' g''$.

These things being premised, I proceed to explain the consequences that must arise from the mixture of the transmitted with the originally reflected rays. The first is, that the rays which after transmission re-enter the prism at different points, and are the cause of the streaks, will not proceed in a parallel direction with those that by reflection from the same or neighbouring points form the blue bow. For instance, let A α α', and B β β', fig. 13, be two incident and reflected rays of the blue bow; then if the yellow ray transmitted at α after two refractions, and one reflection, not expressed in this figure, re-enters the prism at y, it will make the angle $y'y$ F equal to the angle A α G. But from the construction of the blue bow, it has been shown that B β G is greater than A α G; β' β F is therefore greater than $y'y$ F, and the rays β β' and y y' will meet somewhere in the line β β' produced. If we call the greatest of the two angles m, the smallest n, and the distance of the angular points d, then $d \times \frac{\text{sin. } n}{\text{sin. } m - n}$ will give us the length of the line β β', at which the two rays will meet and intersect each other, which according to the enlarged size of this figure, will be at 778 inches from β. For the same reason the orange ray o o' will meet β β' at 1084 inches, and the red ray r r' at 1401 inches from β. It follows also from the same construction, that some of the transmitted rays will diverge from the reflected ones; for instance, the green ray transmitted at α, which re-enters the prism at g, will make the angle $g'g$ F less than the angle β' β F; the rays β β' and g' g will therefore diverge. To this may be added, that g g' y y' o o' r r' and α α' will be parallel.

If such difference between the directions of the transmitted

and reflected rays takes place, it will be seen that the rays transmitted through different points are among themselves subject to the same variety in the direction of their course; $r'r''$, $o'o''$, $y'y''$, $g'g''$, for instance, which passed through the point α, are parallel to each other; but all of them converge respectively to rr', oo', yy', gg' transmitted through c; and on the other hand $y'y''$, $o'o''$, $r'r''$, diverge from gg'.

Fig. 14, Plate XIV. is a general representation of the course of the rays of the blue bow, and of those that produce the streaks. The base of the bow is divided into twenty equal parts, and one ray of the bow reflected from each of the points of the division is marked by a line. Twenty-one sets of rays of the different colours transmitted through the same points re-enter the base at their calculated places, and are represented by dotted lines drawn at proper angles; but here it should be noticed, that the difference of the twenty angles being much too small to give any idea of their converging or diverging condition, the difference between each set has been expressed by one degree less towards the right, and one degree more towards the left; the angle of the middle ray being of its proper magnitude. The strong lines marked A α, B β, C γ, D δ, E ϵ, show the division of the colours, and are the same which in fig. 1 were used to explain the construction of the blue bow. The rays incident on the base FG, in the direction of these lines, which are reflected in the same angles, and are also marked with strong lines, meet at the point where the eye is supposed to be placed.

The figure has been drawn by the result of a strict calculation contained in the following table. In the first column are the angles of the obliquity of the incident rays; in the second

we have the distances of the reflecting points on the base from α. The remaining columns contain the distances also reckoned from α, at which the transmitted rays of the several colours re-enter the base, after two refractions and one reflection.

Table of Calculations.

No.	Obliquity.	Distances.	Red.	Orange.	Yellow.	Green.	Blue.	Indigo.	Violet.
	° ′ ″								
1	α 49 46 12,50	,0000000	,0019198	,0022974	,0026675	,0043053			
2	49 47 17,58	,0012397	,0030962	,0034310	,0037464	,0049503			
3	49 48 22,65	,0024794	,0042782	,0045780	,0048491	,0057881			
	β 49 49 28,00	,0035880	- - -	- - -	- - -	- - -			
4	49 49 27,73	,0037191	,0054653	,0057356	,0059726	,0067327			
5	49 50 32,80	,0049588	,0066506	,0069021	,0071117	,0077443	,0118750		
6	49 51 37,88	,0061985	,0078517	,0080759	,0082630	,0088005	,0112212		
7	49 52 42,95	,0074382	,0090501	,0092559	,0094243	,0098887	,0115764		
8	49 53 48,03	,0086779	,0102514	,0104413	,0105938	,0110005	,0122780		
9	49 54 53,10	,0099176	,0114553	,0116311	,0117701	,0121302	,0131457		
	γ 49 55 34,00	,010721	- - -	- - -	- - -	- - -	- - -		
10	49 55 58,18	,0111573	,0126614	,0128249	,0129523	,0132741	,0141088		
11	49 57 3,25	,012397	,0138695	,0140221	,0141395	,0144294	,0151325	,0186142	
12	49 58 8,33	,0136367	,0150797	,0152224	,0153309	,0155939	,0161973	,0183685	
13	49 59 13,40	,0148764	,0162915	,0164254	,0165261	,0167661	,0172916	,0188450	
	δ 49 59 41,00	,0154406	- - -	- - -	- - -	- - -	- - -	- - -	
14	50 0 18,48	,0161161	,0175048	,0176307	,0177246	,0179447	,0184081	,0196009	
15	50 1 23,55	,0173558	,0187194	,0188382	,0189259	,0191288	,0195415	,0204987	,0231720
16	50 2 28,61	,0185955	,0199354	,0200476	,0201299	,0203177	,0206884	,0214808	,0231409
17	50 3 33,70	,0198352	,0211525	,0212588	,0213301	,0215106	,0218460	,0225168	,0236904
18	50 4 38,78	,0210749	,0223707	,0224715	,0225444	,0227070	,0230125	,0235906	,0244812
19	50 5 43,85	,0223146	,0235899	,0236857	,0237545	,0239066	,0241864	,0246915	,0253990
20	50 6 48,93	,0235543	,0248100	,0249013	,0249663	,0251090	,0253664	,0258130	,0263933
21	50 7 54,00	,024794	,0260309	,0261180	,0261797	,0263138	,0265518	,0269504	,0274380

From the complex nature of this figure, it will immediately be seen that we cannot attempt an investigation of the particular streaks, that will be formed by the mixture of the transmitted with the reflected rays. An inspection of it, however, will be sufficient to show that streaky appearances must be produced. For instance, between a and the first red ray which re-enters the base, a narrow blue streak should be seen; this will be broken in upon by the mixture of two sets of red, orange, and yellow rays, which together with the reflected colours of the bow, the green being still wanting, must give a pale red division immediately joining the blue streak. When we advance farther into the figure, the great mixture of the colours and the different directions of the rays are so various, that nothing particular can be determined without entering into a very complicated calculation of the meeting and intersections of the rays; we see, however, that these mixtures will produce a condensation of rays in some parts, and vacancies in others, so that no uniform tinge can remain, and consequently streaky appearances must be seen. The same conclusion may be drawn from an inspection of the places where the transmitted colours re-enter the base; for the green, which is transmitted between α and β does not enter again till after the fourth division of the base; the blue which begins to be transmitted at β cannot find admittance again till after the tenth; the indigo transmitted from γ to δ does not re-enter into the composition till after the sixteenth division; and the violet transmitted between δ and ϵ will only come in again after the nineteenth. There will consequently be a considerable space without green, another without blue, a third without indigo, and a fourth without violet; from

which it follows, that streaky appearances must every where be seen in the composition of the rays that come to the eye. We should also notice that towards δ all colours but violet will be transmitted, for which reason when they rise again a compound of them will produce streaks that approach to white, such as pale red, pale bluish green, dingy yellow, and dirty white; so that both at the beginning and end of the bow-streaks all observations * of them agree perfectly with what is pointed out by the foregoing remarks; and though we have not analysed the particular construction of the streaks in the middle of the bow, yet what has been said will sufficiently prove that various successive changes of the colours must also take place.

It will be understood that I have only attempted to give some idea of the action of surfaces, in giving configuration to colours that are already produced; but that the principle of reflection is the cause of streaks will remain evident, even if the method of its action should not have been explained so much to our satisfaction as we might wish. It will also remain to be proved, that streaks are only the effect of one of those modications which depend on the figure of the reflecting surface;† and having got thus far in this research, I may advance towards a final consideration of my subject.

49. *Prismatic Bows when seen at a Distance are straight Lines.*

The next point to be shown, in order to approach gradually to a solution of my problem, is that the apparently arched figure of the blue and red bows, which may be seen in a prism,

* See the first paragraph of the last article.

† See the second paragraph of the 44th article.

is merely the consequence of the position of the eye, and the modifying power of the surface through which it sees them. For a proof of this, it would be sufficient to refer to the principles of the formation of the bows, from which it must be evident that the critical separation of the rays will be exerted in every direction, and that the extent of the bows we see would consequently be parallel to the sides and base of the prism, if the eye could receive the rays which form them, every where in the same angle from a line drawn parallel to that side of the prism through which they pass. An experimental confirmation of this we have by laying down a prism, and keeping either of the bows in view while we gradually draw the eye away; it will then be seen that the curvature, which the bows had assumed, will continually be diminished, and nearly vanish at a very moderate distance.

50. *The Colours of the Bow-streaks owe their Production to the Principle of the critical Separation of the different Parts of the prismatic Spectrum.*

That streaks will be produced when a plain glass is laid under the side of a prism which forms either of the coloured bows, has already been sufficiently shown; but that these streaks, as well as the rest of the phenomena which have been mentioned in the 44th article, are exclusively to be deduced from the same principle by which the bows have been explained will require some proof. With regard to streaks, the following experiment, I believe, will remove every doubt upon the subject.

Let a plain glass be laid under the base of a right angled prism; then, if the eye at first be placed very low, no streaks

will be seen; but when afterwards the eye is gradually elevated, till by the appearance of the blue bow we find that the principle of the critical separation of colours is exerted, the streaks will become visible, and not before; nor will they remain in view when the eye is lifted higher than the situation in which the effects of the critical separation are visible. It is therefore evident, not only that the colours are furnished by the same cause which produces the bow, but also that they are modified into streaks by the plain surface under the prism.

In addition to this, it must be remarked that the criterion, which has been successfully used in the explanation of several prismatic phenomena, proves that no other colours, but those which arise from the same source, can be modified so as to give streaks. The following experiment will show the foundation on which this criterion is established.

Let there be an horizontal opening in the upper part of a window-shutter, of about three feet long and one foot high; then, if we look at it through one side of a right angled prism, we shall see a red bow from the highest margin of the opening, and a blue one from the lowest; but when a plain glass is applied to either of the sides of the prism through which we see these bows, neither of them will give any coloured streaks. The experimenter must carefully keep the critical bows out of the way; for should either of them fall upon those which are under examination, streaks must of course be seen to pass over them.

When a spherical surface is placed under the prism, it has likewise been shown that coloured rings will be seen; but these, like the streaks, will not be visible when the eye is

below the place where the bows can be seen, which would not have happened had a plain glass been used instead of the prism; for with such an arrangement, coloured rings may be seen at the most oblique as well as perpendicular stations of the eye.—As soon as the blue bow is perceived, the rings begin to be formed, first partly, then half, and lastly, we see them completed; and what is remarkable, these coloured rings are of such a magnitude and brightness, that they cannot be a moment mistaken for those we see when a plain glass is laid upon the same spherical curve.—The eye being then gradually elevated above the range in which the bows may be seen, these rings will pretty suddenly shrink in their dimensions, and lose much of their brilliancy; till at last, when the eye comes to a perpendicular situation, we find them dwindled away to the size and appearance of such as may be seen when a plain glass is substituted for the prism.

Irregular surfaces are no less decisive in the phenomena they exhibit; for when an equilateral prism is laid upon red mica in a strong illumination of scattered light, we may see a most admirable variety of very minute coloured appearances, whenever the eye is brought to the blue bow place; but as soon as it is in the least elevated above, or depressed below that situation, these fantastical figures are sure to vanish.

31. *A Lens may be looked upon as a Prism bent round in a circular Form.*

Those who have followed me in the analysis of the blue and red bows, will readily enter into the application I shall make of this theory to the generation of coloured rings by lenses.

It has been proved, that the different refrangibility of the prismatic colours, at certain critical angles, will cause the violet; indigo, blue, and part of the green rays to be separately reflected, and that, according to what has been said in the 49th article, this will produce an extended straight-lined appearance tinged with the abovementioned colours. It has also been shown that the same principle, at certain critical angles, will cause the red, orange, yellow, and part of the green rays to be exclusively intromitted, in such directions as will produce a similarly extended straight-lined appearance tinged with these latter colours. From the angle in which the eye must receive these appearances in a prism, they are converted into the blue and red bows; but, since they would appear to be straight lines, if they were seen in directions perpendicular to a line drawn parallel to the edges of the prism, it follows, that were a long prism bent round into a circular form so that its two ends might meet, these lines would then be changed into rings, one of which would be formed by reflection, the other by transmission.

A lens may be said to be such a prism, from which indeed it differs only in one respect, which is, that an angle contained between two lines applied as tangents to different parts of its surface is changeable, whereas the refracting angle of a given prism is constant.

If it should be remarked that in consequence of considering a lens in this light, a plano-convex one, for instance, ought to present us, in certain situations, with a ring of the colours of the blue bow, and in others with a similar ring containing those of the red one, I must observe that the reason why such rings or bows can never be seen by the eye, though the phy-

sical separation of the rays should actually take place, is owing to that particular circumstance in which, we have remarked, the lens differs from a prism, namely, the curvature of the refracting surface; for although it has been proved that the figure of the first surface of a prism is not concerned in the formation of the blue bow, yet that of the surface through which it is seen by the eye is of material consequence, as will appear by the following experiment.

An equilateral prism, one side of which I had made cylindrical, was exposed so as to receive the incident light through the convex surface. In this situation, the eye being about three or four inches from the prism, a bow was formed which in every respect was like one I saw in another equilateral prism, whose three sides were flat; but when the convexity of the first prism was turned towards the eye, the bow could no longer be seen, although the critical separation of the rays would undoubtedly form it in this, as well as in the other prism; the two sides and angles of each exposed to the light being perfectly equal. By much attention to what may be perceived when the eye is placed at various distances, I found that the curvature of the surface through which I tried to see the bow, produced a focal contraction and subsequent inversion of the rays in their passage to the eye, and thus occasioned a total change of appearances. Now, since a ring or bow would not be visible in a prism bent round, if the side through which it must be seen were curved, we cannot expect to see such appearances in a lens, which every where presents us with a spherical surface.

The effect upon the appearance of the bows, produced by the surface through which the rays must pass to come to the

eye, may be still better examined by laying the plain side of a plano-convex glass of a short focus upon the flat side of a prism, through which we see either of the bows; for when the eye is near the focus of the lens, they will be entirely effaced as far as they are covered by the lens.

A consequence of great importance may be drawn from these experiments; for since the cause of the coloured appearances, which have been called bows when seen in a prism, is now perfectly understood to be the critical separation of the colours of the incident light, it must be admitted that such a separation will certainly take place whenever a beam of light can find an entrance into glass, so as to make the required angles either with an interior or exterior surface, be it in the shape of a prism, lens, or solid of any kind, although the figure of the last transmitting surface should not permit such coloured-appearance-making-rays to reach the eye. A plano-convex lens will consequently by its construction separate the rays of light which enter at the convex surface in such a manner, as by reflection to produce what, if it could be seen, would be called a blue bow, and by rays that come in at the plain side, separate them by intromission so as to produce a red one.

To remove all doubt about the truth of this theory, I ground a small part of a plano-convex lens flat, that I might look into it, as it were, through a window, to see what passed within. The flat made an angle with the base of about thirty-four degrees, and I saw through it very plainly, in different directions of the illumination, a blue bow by light entering at the convex surface, and a red bow by light coming in at the plain one.

With regard to a plain glass contained between parallel

surfaces, it may be remembered that when in the last paragraph of the 39th article I said that streaks could not be seen by laying another plain glass under it, I intimated at the same time the formation of colours; this will now admit of a satisfactory explanation. Scattered rays will enter into a parallel piece of glass, and by reflection the critical separation of colours will take place on its interior surface, so that if this effect could be seen, a blue bow would appear; and in the same manner a red bow might be seen by rays intromitted through the lowest surface. In consequence of the course of these rays, streaks would also appear from each of the bows when another plain glass is laid under the parallel piece; but from a calculation made according to the principles that have been established in the preceding part of this paper, the reflection of a mean ray of the blue bow from the interior surface being at the angle 49° 57′ 3″.3; and this being also the oblique incidence on the upper surface, a ray which comes in that direction with the mean refrangibility of the rays of the blue bow cannot come out of glass. The angle of obliquity of the mean intromitted ray for the red bow is 49° 38′ 19″.5, and on computing its direction by the mean refrangibility of the red bow, it will also be found that it cannot clear the glass. I have seen the bows and their streaks when the upper surface of the glass was inclined only nine degrees to the lower one; and possibly a much smaller angle would have been sufficient to permit the emergence of the coloured rays. The strong reflection from the outside of the glass, and the contraction of the dimensions of the bows are however much against perceiving them at a great obliquity.

52. *The critical Separation of the Colours, which takes place at certain Angles of Incidence, is the primary Cause of the Newtonian coloured Rings between Object-glasses.*

It has been proved that streaks, concentric rings, lenticular figures, and all sorts of irregular coloured phenomena may be seen by means of the prism; and in the 35th, 36th, and 37th articles, it has already been sufficiently explained that the cause of the great variety of these appearances is to be found in the configurating power of surfaces. I have also remarked in the 40th article, that in order completely to account for the Newtonian rings, it remained only to be shown how the colours thus modified are produced.

The prismatic experiments contained in this paper have explained in what manner a critical separation of the colours, which takes place at certain angles of incidence, is the cause of the appearance of the blue and red bows; since the different reflexibility of the rays of light, by which Newton has accounted for the blue bow, brings on a critical separation of the blue colours, and since also the different intromissibility by which I have explained the red bow, occasions an equally critical separation of the red ones.

In the 50th article I have not only proved that all the above described various appearances, which in the first part of this paper were produced by convex glasses, may be equally well obtained by the use of a prism, but have also shown that the great simplicity of this valuable optical instrument has cleared up great difficulties, by pointing out to us that the colours which are modified into such various shapes, are in all prismatic experiments exclusively produced by the critical separation

of the rays of light. Now, as this must be admitted; it will certainly not be philosophical to look for a different cause of the same or similar effects, when convex glasses, which have all the required prismatic properties are used to produce them.*

To show the great similarity, or rather the identity of these effects, let us examine them in different points of view, and since the variety of the configurations is no longer an object that wants explaining, I shall only take the most simple case of each, namely, the coloured rings, that are produced when a plano-convex lens is laid with its convex side upon a plain reflecting surface; and the coloured streaks which are produced when the base of a right angled prism is in the same manner placed upon such a surface.

The form of rings arises from the spherical figure of the lens.†

The right-lined appearance of the streaks is owing to the straight figure of the plain surface of the prism.‡

The colour of the rings may suddenly be changed.§

The colour of the blue bow-streak may as instantly be converted into those of the red bow.||

The cause of the sudden change of the rings has been shown to be that the sets of one colour are seen by reflection, and those of other by transmission.¶

* By this it will be understood that if any case should occur, in which the critical separation cannot account for the observed phenomena, we are then authorised to look out for some other cause to explain them.

† See the first paragraph of this paper.

‡ See the 49th article.

§ See the 15th article of the first part of this paper.

|| See the 43d article.

¶ See the 8th article of the first part of this paper.

*It has also been shown that the blue bow-streaks are seen by reflection, and those of the red bow by transmission.**

In a lens we may at the same time see, in half the set, the colours of the reflected, and in the other half, the colours of the transmitted rings.†

And in a prism held before an open window, when the eye is close to it, and when half the bow falls on the side of the room, we may see blue streaks by reflection from half the blue bow, and green streaks by transmission from half the red bow. ‡

When deep convex, or concave glasses, are laid upon the first surface of a lens, the rings are not affected by it.§

And when the same glasses are laid upon the first surface of a prism the streaks remain unaltered. ||

When the convexity of the lens, which is placed on the reflecting surface, is changed, the size of the rings is also changed.¶

*And when the angle of the prism is increased or diminished, the distance of the streaks undergoes a proportional alteration.***

When the lens is pressed upon the plain glass, the rings increase in diameter.††

And by a pressure of the plain glass against the prism the distance of the streaks grows larger.

To form rings by a lens, scattered light is only required.‡‡

And the same light is best for the production of streaks by a prism.§§

Many other instances of similarity might be adduced, but

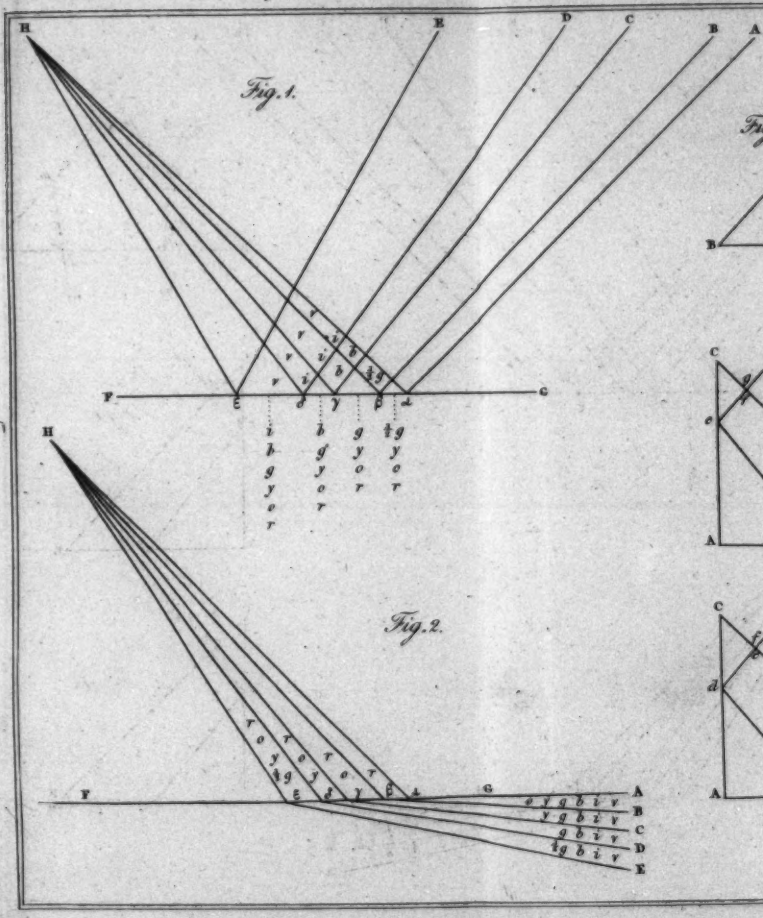
* See the 43d article. † See the second paragraph in the 18th article.
‡ The experiment has been made, though not mentioned in this paper.
§ See the sixth paragraph of the 24th article.
|| See the second paragraph of the 46th article.
¶ See the first paragraph of the 7th article.
** See the fourth paragraph of the 42d article.
†† See the 8th article. ‡‡ See the seventh paragraph of the 24th article.
§§ See the third paragraph of the 46th article.

those that have been recited will surely be sufficient to show that the same operations, which will produce these prismatic phenomena, will equally account for those that are formed by the lens; now, as it has been clearly proved, that the critical separation of the colours, which takes place at certain angles of incidence, occasions all the phenomena of the blue and red bows, and of the streaks, rings, and other regular or irregular appearances, that may be seen in a prism, there cannot remain a doubt but that the Newtonian rings observed between object glasses, are owing to the same cause.

53. *Remarks relating to the Newtonian alternate Fits of easy Reflection and easy Transmission.*

In attempting to rescue the science of optics, from what has been so long considered as unsatisfactory for explaining the great question about the cause of the coloured rings, I have made use of a principle, the effects of which have so near a resemblance to those of the suppositious fits of easy reflection and easy transmission, that the author of them might easily be misled by appearances. But although the principle of a critical separation of the colours substituted for these fits, admits the reflection of some rays at the same angles of incidence at which others are transmitted, yet since the Newtonian different refrangibility of light will account for these critical reflections within glass, and the equally critical intromissions from without, we can have no longer any reason to ascribe original fits to the rays of light, which in the first part of this paper, they have already been proved not to possess, and which now, in all prismatic experiments, I have shown are not necessary for explaining appearances that may be accounted for without them.

Slough near Windsor,
Dec. 9, 1808.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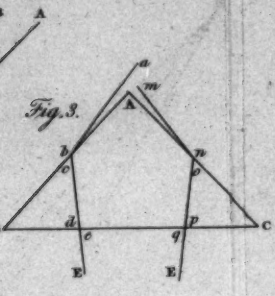

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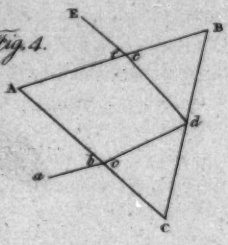

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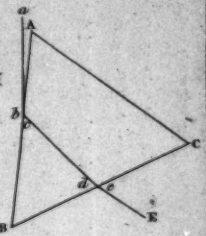

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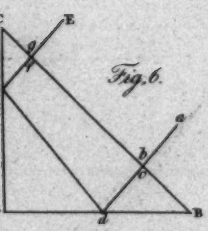

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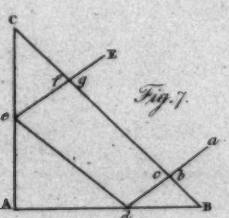

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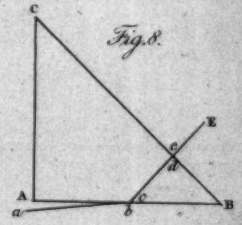

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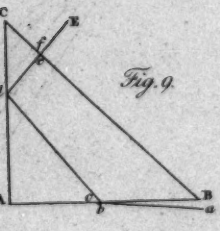

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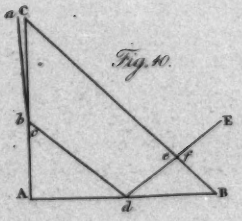

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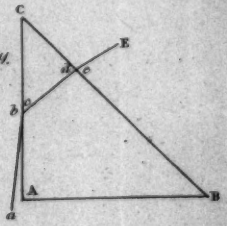

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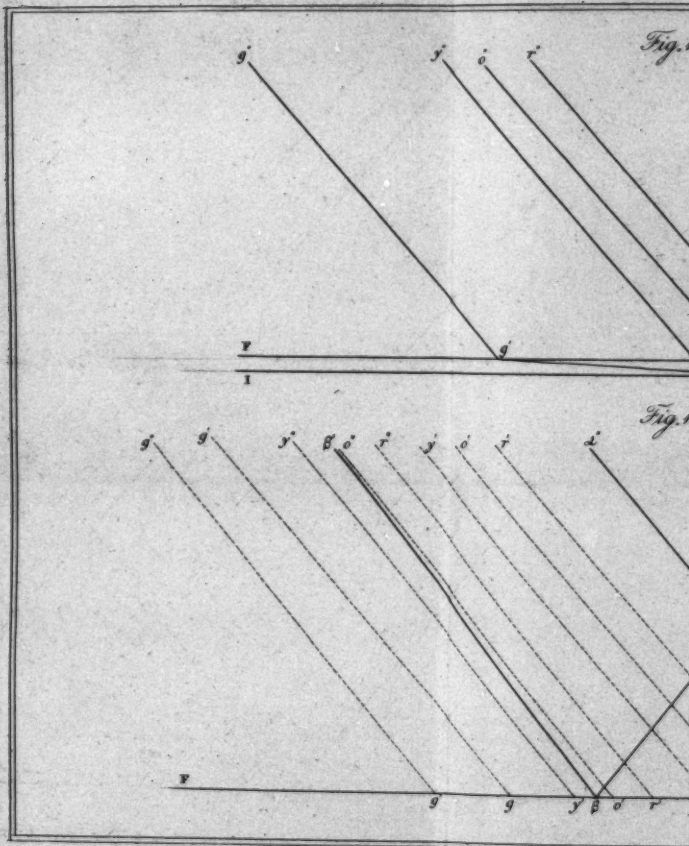
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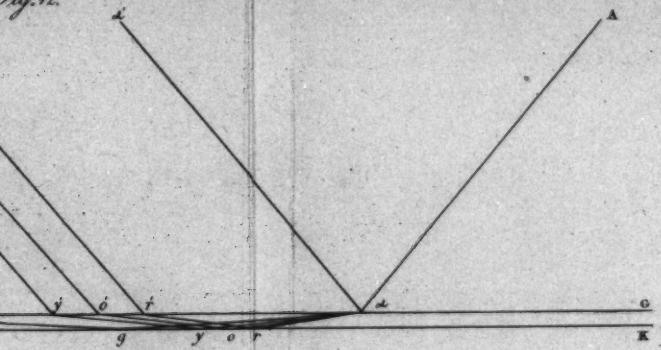

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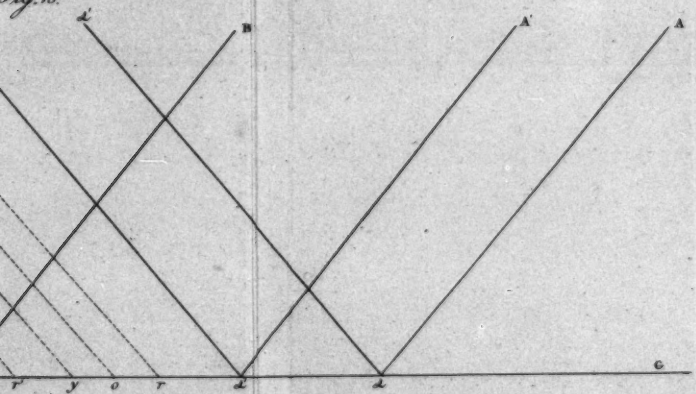

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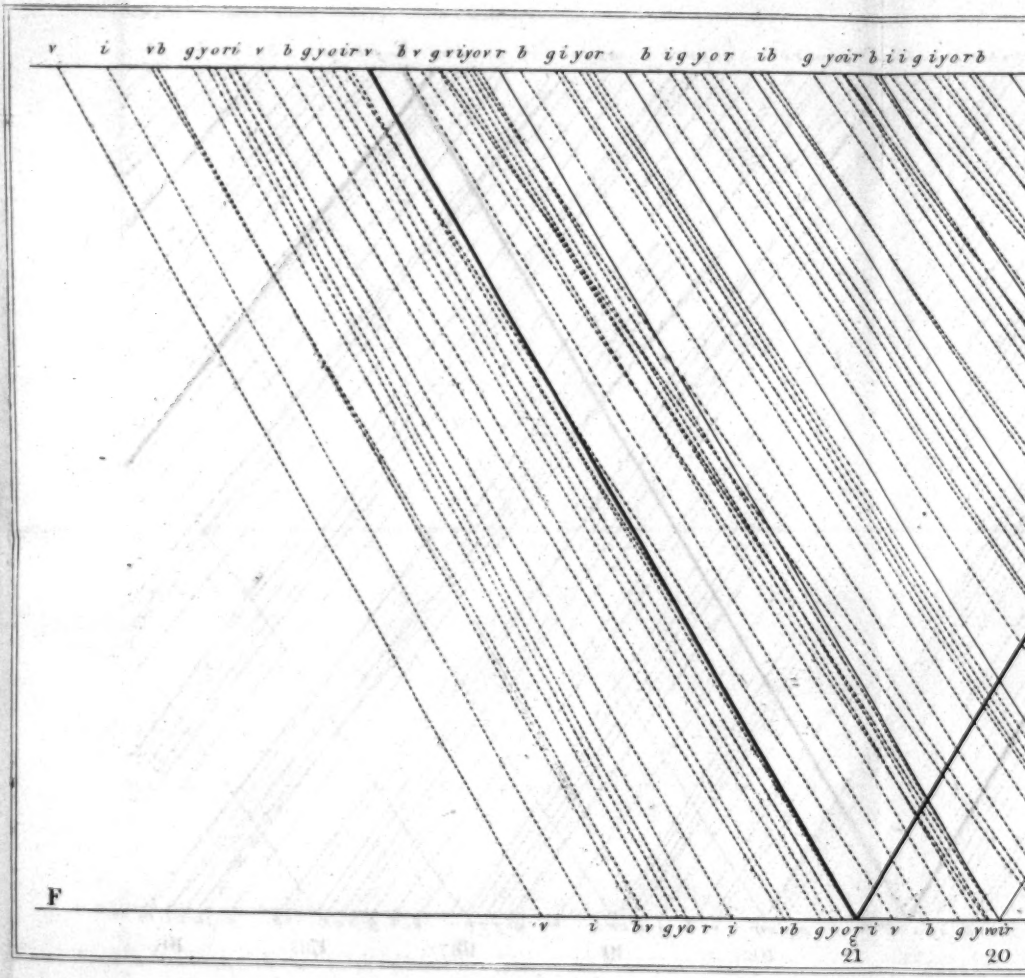
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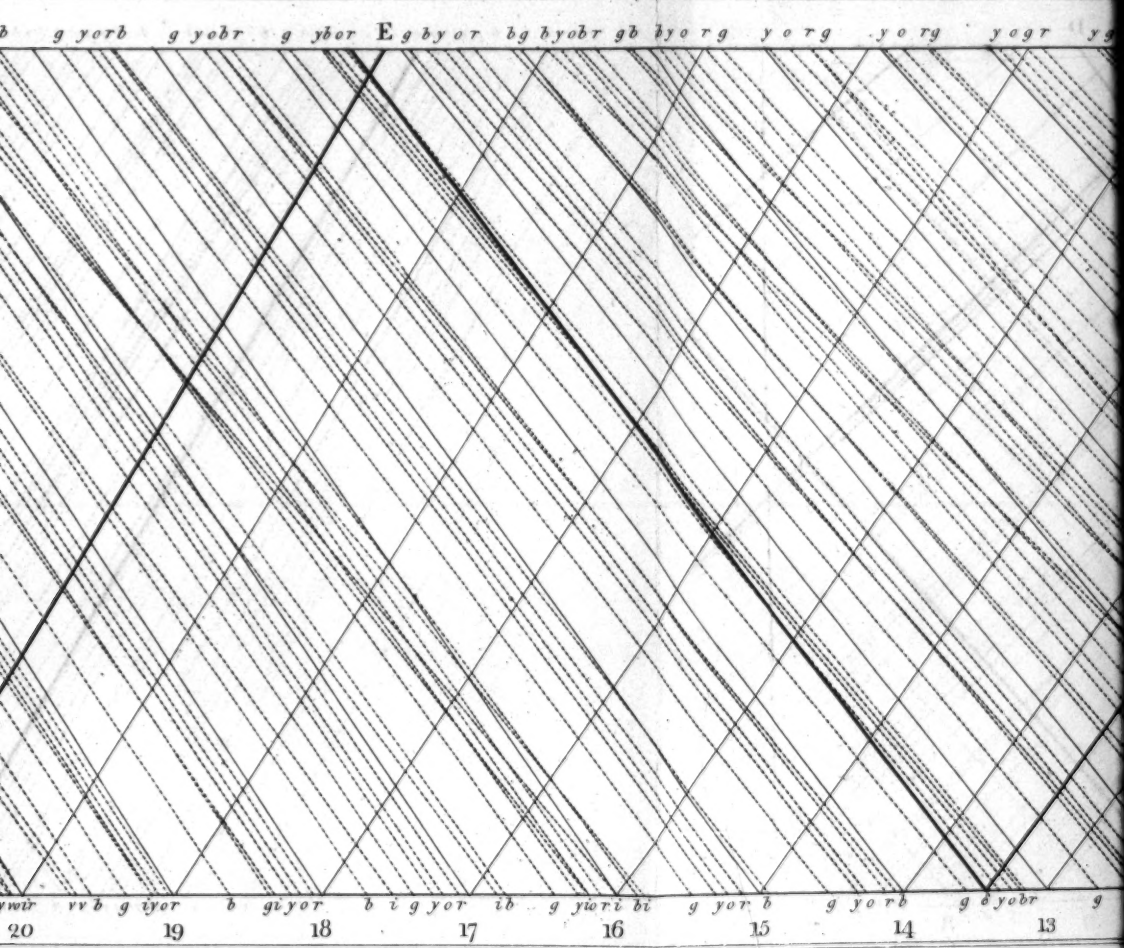
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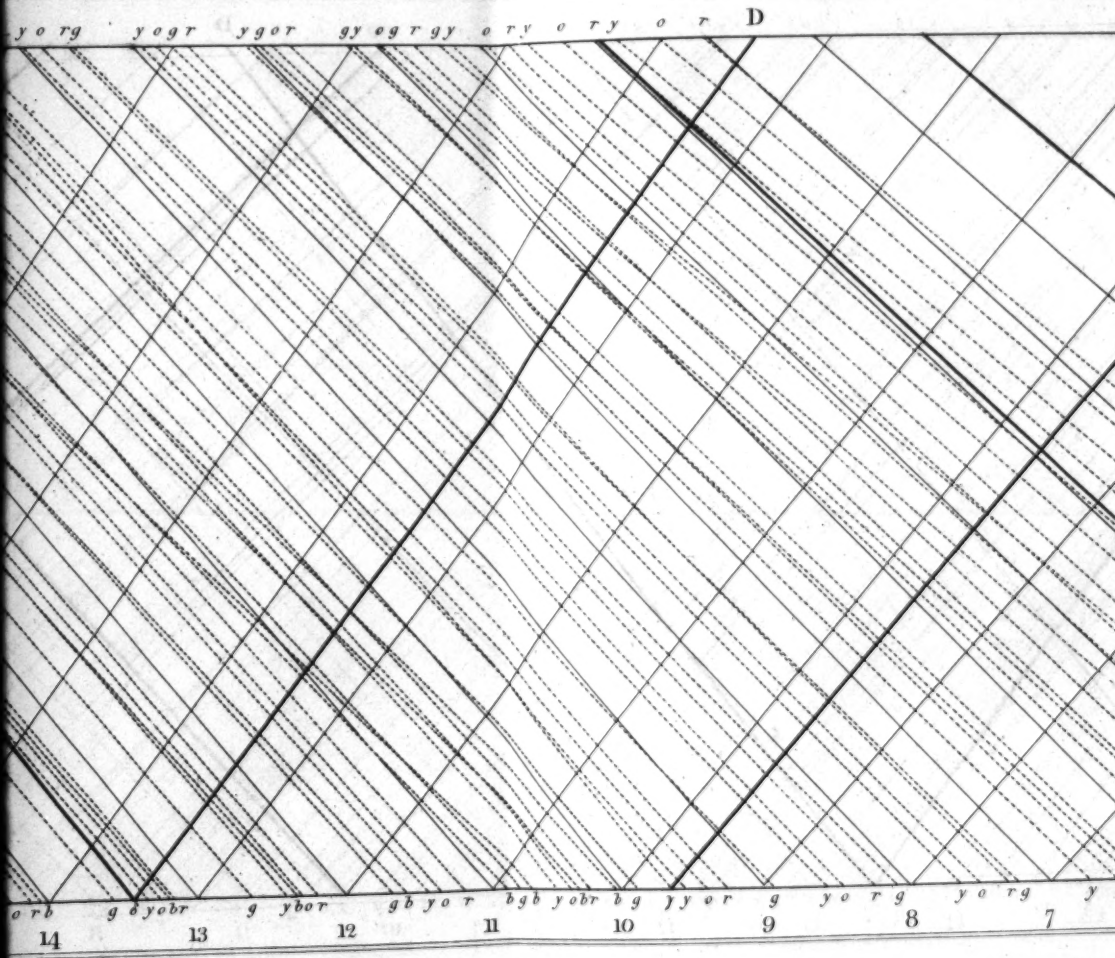
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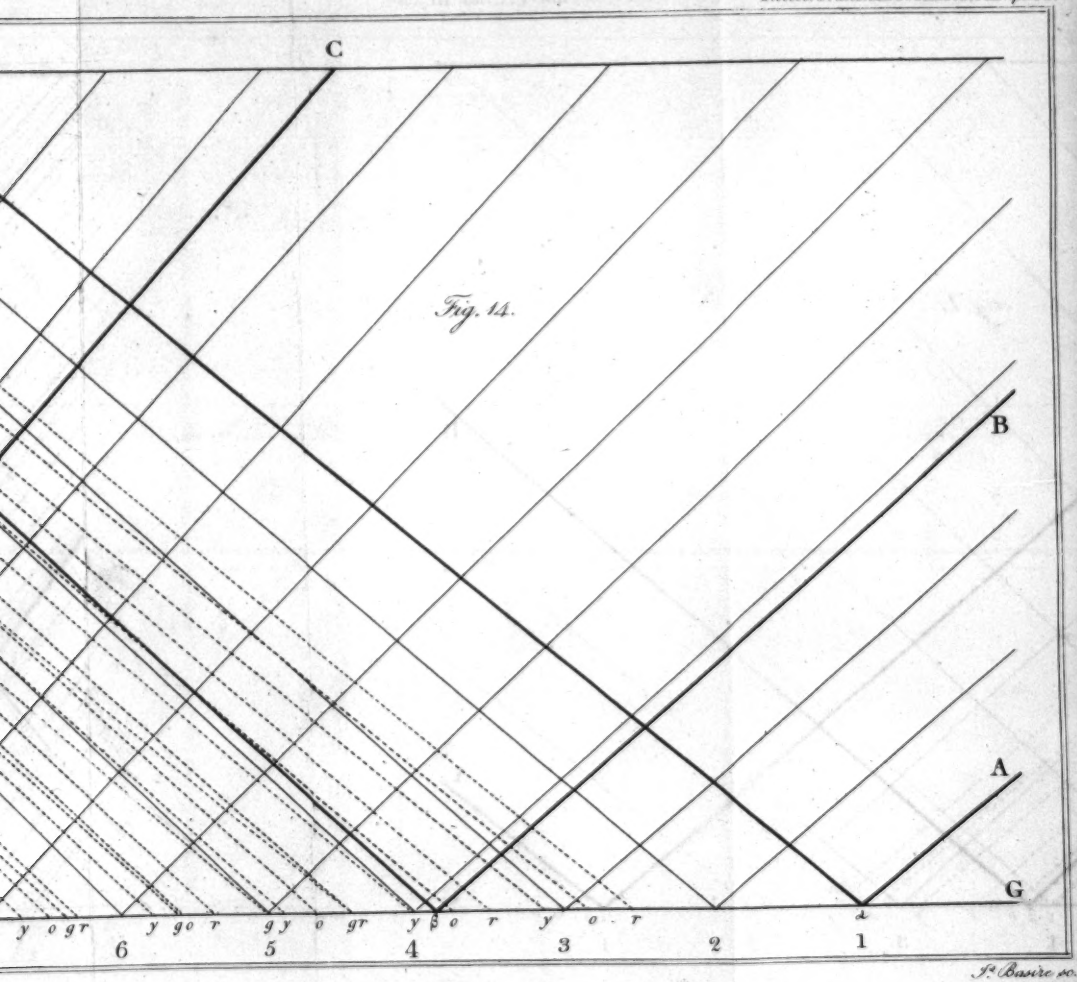


Fig. 14.

J.e Basire sc.

XVIII. *An Account of a Calculus from the Human Bladder of uncommon Magnitude.* *By Sir* James Earle, *F. R. S.*

Read June 15, 1809.

Sir Walter Ogilvie, Bart. of Dundee, an officer in the regiment of Scotch Greys, at the age of twenty-three, active and healthy, was crossing the ferry at Leith, when he received a blow on his back, from the boom of the vessel, which paralized the pelvis and lower extremities. During two months he was obliged to have his water drawn off; for fourteen months he remained in bed, or in a horizontal posture, and though he then recovered the use of the bladder, and of his limbs, sufficiently to walk across the room with the help of crutches, and also to ride, when placed on an easy low horse, his health continued many years in a weak and precarious state, while the limbs acquired little additional strength or powers.

About twenty years after the accident, perceiving symptoms of stone in the bladder, he was examined by Mr. Benjamin Bell at Edinburgh, and a stone was felt, which was judged to have attained a considerable size; the operation of extraction was then recommended, but was postponed from time to time, though his health declined, and the irritation and pains in the bladder gradually increased.

Sir Walter continued to endure this state of existence twenty-eight years from the time of the accident, when he

became unable to make water in an erect position; this inconvenience increased to such a degree, that latterly he could make none without standing almost on his head, so as to cause the upper part of the bladder to become the lower, and this he was obliged to do frequently, sometimes every ten minutes, as the quantity at each time was less than the measure of a wine glass; and when he used exercise, it was tinged with blood.

The principal remedies which had been prescribed for him were aqua calcis and uva ursi; but he never persisted long in the use of either. At times, when the pain was violent, he had recourse to opium by the mouth and per anum: this, added to a naturally costive habit of body, rendered necessary the frequent assistance of aperients.

At the age of fifty-three, thirty years after the accident, the spasms and fits of pain, from the urgent desire to void urine, became so frequent and violent, and his life so completely miserable, that he was determined to have the stone extracted. I received a letter, requesting my opinion whether a paralytic state of the lower limbs was a prohibition to the operation of lithotomy; on my reply to the contrary, he was put on board a ship and conveyed to the Thames, brought in a boat to Hungerford Stairs, and in an easy carriage to Hanover-street, without suffering any inconvenience of material consequence.

Toward the latter end of July, 1808, I visited him, when he gave me a clear and distinct account of what has been related, and added, that the stone could be evidently felt above the os pubis. At first, I much doubted of the large prominent tumor which I saw in the lower part of the belly, being a stone, but on attempting to pass the sound, it would not enter the bladder,

being stopped by a solid mass; and on further examination, I was thoroughly convinced that there was a stone of sufficient size to fill the bladder.

He said he came to England with a determination to have it extracted, if it were possible, and desired my opinion as to its practicability. On such an extraordinary and important case I declined an immediate answer, but requested a consultation; and Mr. CLINE was appointed to meet me. After mature consideration of every circumstance, we were of opinion that the possibility of extraction must depend on the consistence of the stone; if it proved soft, as is well known to be frequently the case, it might be taken away; but if too hard to be broken, it would be too large to be extracted whole, and must be left.

The operation of extracting it above the os pubis was thoroughly considered, and concluded to be uncertain and dangerous, because the bladder, thickened and exquisitely irritable, could not bear to be further distended with fluid, and the stone, although so large, had not raised it sufficiently high to obviate the danger of wounding the peritoneum, and penetrating into the cavity of the abdomen: the usual lateral operation was therefore judged to be the only safe and probable means to be attempted.

After some days consideration, Sir WALTER, thoroughly and perfectly aware of the difficulties which might reasonably be apprehended in the extraction, from the magnitude, and also from the uncertainty of the structure and consistence of the stone, determined to submit to the operation, and Mr. CLINE was requested to perform it.

On Thursday, August 11th, he was placed in the usual

situation, and the proper ligatures were applied; but it was soon found that the lower limbs were so incapable of action or resistance, that they were left unconfined. The staff could be passed in no farther than the neck of the bladder; the division of the urethra and prostate gland was made with the scalpel and probe-pointed bistouri: when this was accomplished, it was found impossible to introduce any kind of forceps; but on pressing hard with the finger, part of the stone felt soft, gave way, and made some room for the forceps, which brought away several portions, and with the assistance of a scoop, as much stone was extracted as would have filled a large tea-cup; but the great mass, beyond what the finger could reach on either side, still remained hard and impenetrable, and after repeated trials with forceps of different kinds, and of the strongest powers, it was found impossible farther to reduce the size of it, or take it away.

The patient bore the several attempts, and the necessarily protracted operation, with great firmness; probably, from the paralized state of the parts, the natural acuteness of sensation was blunted; however, as from weakness and fatigue he was becoming much exhausted, and the complete extraction appeared clearly out of our power, it was judged right to relinquish any further endeavours.

No hæmorrhage ensued, he became calm and composed, and passed a tolerably good night; the next day he complained only of the same kind of spasms, and frequent pressing desire to void urine that he had been accustomed to feel, and not in any very great degree more acute. As some proof of this, he was frequently inquiring when I thought the wound would be in a state to admit of the extraction of the remainder.

The second night passed nearly the same, without tension of the belly, the urine flowing through the wound, and some by the penis; the third day he complained much of the tenderness of the abdomen, which was tense and painful, and peritonitis seemed rapidly to be taking place, which, however, was lessened and quieted by bleeding and fomentations, and he again became easy: this state continued for several days, but he complained much of the frequent returns of his former spasms, after each of which a small quantity of urine was evacuated. He was nourished with broths, jellies, &c. but would take nothing solid, not even bread in any form; after the fifth day, he ate a few oysters, some fish, or chicken, drank occasionally of porter, and his health and strength were improving; but though these favourable symptoms continued, with the abdomen soft and easy, so as to bear examination by the hand, and all inflammatory action was subsided, yet the repeated spasms continually broke his rest, and kept him in a constant state of irritation, obliging him to violent efforts in resisting them, and to get instantly on his knees, with his head low on the bed, to enable him to expel the urine; and one spasm frequently succeeding before the former had well subsided, kept his whole frame in continual agitation, to the greatest possible degree that nature could bear. All these sensations and occurrences were very similar to what he had experienced for several months before he left Scotland; and it was then the opinion of Mr. Stewart, a medical gentleman who accompanied Sir Walter to England, that his complaints were increasing to such a degree, that it was scarcely possible for him to exist much longer. In his present situation, of course, such shocks and disturbances of the whole animal economy acted with

increased effect, in proportion as he became less capable of supporting them. Toward the eighth day from the operation, he was visibly growing weaker, his pulse smaller and quicker, his little inclination for food became less, and he was with difficulty prevailed on to take any; some cordial medicines, however, in some degree revived him; but on the ninth day he grew more impatient, feverish, and restless, and on the twenty-first of August, ten days after the operation, he desired not to be teased to take any thing more; when, covering himself completely with the bed-clothes, he quietly resigned a most singularly miserable existence.

Examination after Death.

On opening the abdomen, the bladder was found much diseased and thickened, firmly embracing a stone of extraordinary magnitude, and appearing to be completely filled with it. On dividing the bladder from the os pubis backwards to the rectum, the stony mass was uncovered, which I attempted to take away with the largest forceps; but it was impossible. It was then raised by getting the hand under it, with considerable difficulty, as the cohesion between the bladder and the stone was very strong, though there did not appear to be any diseased or distinct adhesions. When taken out, the form of the stone appeared to have been moulded by the bladder; the lower part, having been confined by the bony pelvis, took the impression of that cavity, and was smaller than the upper part, which having been unrestricted in its growth, except by the soft parts, was larger, and projected so as to lie on the os pubis.

A large excavation had been made in the lower part, which

lay on the neck of the bladder, by the operation. The internal structure was thus exposed, in which appeared distinct stones or nuclei, now consolidated into one mass, disposed in layers.

The weight of the stone was forty-four ounces, or three pounds four ounces (apothecary's weight), the form of it elliptical, the periphery, on the longer axis, sixteen inches, on the shorter fourteen.

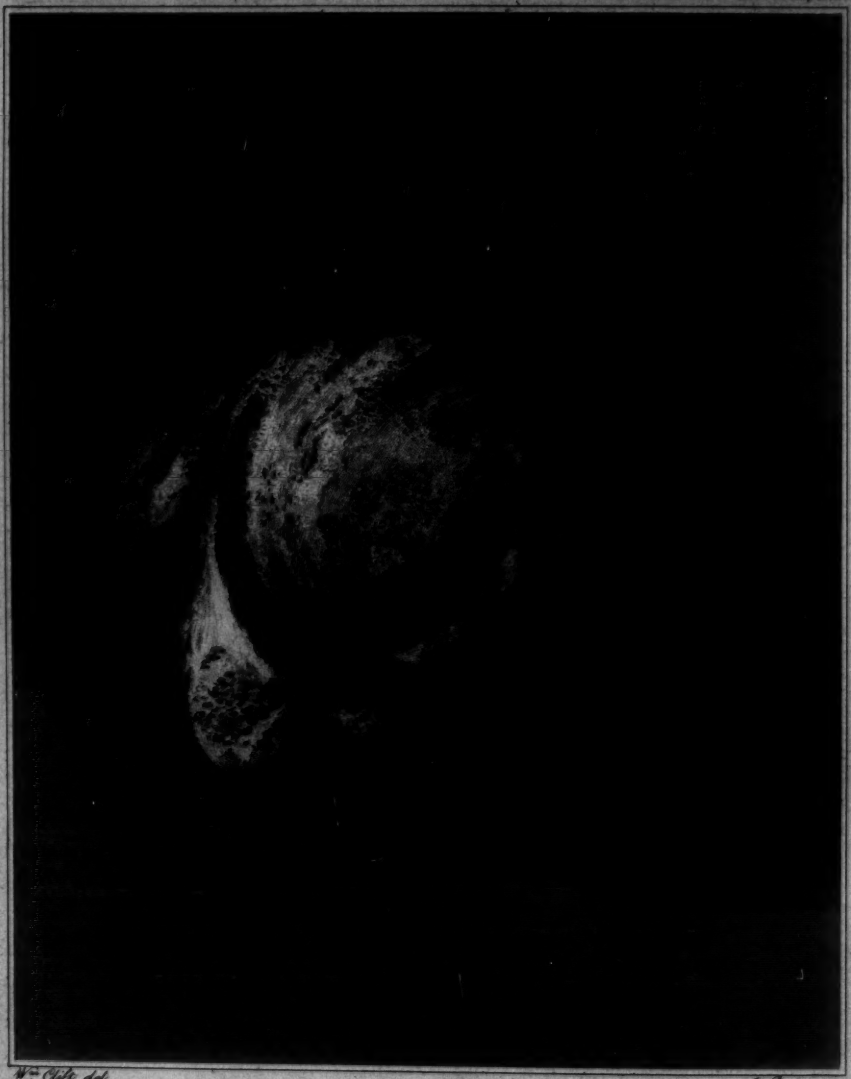
The kidneys were altered considerably in their texture, and their pelvis much enlarged, the left was pressed up higher than natural, and adhered firmly to the spleen. The right was attached to the ascending colon, and general adhesion had obtained between all the surrounding parts. The ureters were much increased in their dimensions and thickness, and were capable of containing a considerable quantity of fluid; they were in fact supplemental bladders, the real bladder having become nothing more than a painful and difficult conductor of urine, which trickled down in furrows formed by it on the superior surface of the stone. This clearly explained the cause which obliged the patient, when compelled to evacuate urine, to put himself in that posture which made the upper part of the bladder become the lower, by which means a relaxation or separation was allowed to take place between the bladder and the stone, so that the ureters had an opportunity of discharging their contents; when the body was erect, their mouths, or valvular openings, must of course have been closed, by the pressure of the abdominal viscera on the bladder against the stone.

His difficulty of voiding urine, appears to have gradually increased as the bladder became more and more filled with the stony matter, and the extremity of his distress did not come

on till the whole cavity was nearly choked up. From the appearances on the examination, it is more than probable that the mouths of the ureters would have been soon completely closed, when a total, and consequently fatal suppression of urine must have taken place.

The disease probably originated when the patient was obliged to continue such a length of time on his back, in which position the surface of the water only may be supposed to have been, as it were, decanted, and the bladder, seldom, if ever, completely emptied; thus in a constitution, perhaps naturally inclined to form concretions, the earthy particles subsided, and by attraction soon began to lay the rudiments of a stone, which was not felt above the brim of the pelvis till many years after, but from that time the gradual increase of it was perceptible to the patient, and his medical friends: from this circumstance, as well as the shape of it, the stone evidently appears to have commenced within the pelvis, and in the lowest part of the bladder.

The texture of it appeared different from the generality of calculi, and to contain more animal matter. In a short time it became highly putrid and offensive: after macerating a few hours in fresh water, the saline particles in its composition began to separate, large flakes separated from its surface, and I was convinced, that if the maceration had been continued, the whole of the outer part would have crumbled away; it was therefore suffered to dry, and the putrid effluvia gradually went off. As a proof that animal matter abounded, the calculus not having been protected from the flies, maggots were produced in it at the end of a fortnight, which were destroyed by washing it with spirits.



W.m Clift del.

F. Basire sc.

Dr. POWEL examined its chemical composition, and informs me, that it decidedly consists of the triple phosphat of ammonia and magnesia, with phosphat of lime, forming together the fusible calculus of Dr. WOLLASTON (Phil. Trans. 1797.) mixed with a certain portion of animal matter, which was separated and floated under a membrane-like form, on the solution of the salts in diluted acids. That this composition is demonstrable by all the usual relations to alkalies and diluted acids, and the precipitates from solutions in the latter, by ammonia, and also by its fusion into enamel under the blow-pipe, and that the general external character of the calculus establishes the same fact by its semi-transparent appearance, and regular prismatic crystals of the triple salt, discoverable by a magnifier, between the more compact layers.

The nuclei were examined solely with the blow-pipe, from a wish not to disturb so fine a specimen beyond what had been done by the operation. By this examination they were found less fusible than the general mass, and appeared to contain a larger proportion of phosphat of lime, for by admixture of a portion of triple phosphat of magnesia, taken from another part of the same calculus, they were rendered as fusible as the rest.

This calculus accords entirely with the description given by FOURCROY (Systême de Connoisance Chemique), and confirms his farther observations on this species. "Ce sont aussi les "concretions urinaires les plus volumineuses de toutes; elles "ont depuis le grosseur d'une œuf jusqu'à une volume qui "occupe toute la vessie, en la distendant même considerable-"ment." From this passage, it should seem that similar instances had occurred to Monsieur FOURCROY; but from my

own observation, and from all the information I have been able to collect, no calculus from the human bladder, of such magnitude, has been hitherto exhibited, or described, in this country.

JAMES EARLE.

Hanover Square, Dec. 26, 1808.

The annexed Plates are representations of its size and figure.

Plate XVI. shews the cavity which was made by the instruments.

It is preserved in the Museum of the Royal College of Surgeons in London.

H. Clift del.
J. Basire sc.

XIX. *On expectorated Matter.* By George Pearson, *M.D. F.R.S.*

Read June 15, 1809.

THE attention of physiologists has been very much withdrawn, for the last half century, from the consideration of the different states of the circulating and secreted fluids, in consequence of the opinion that the nervous and fibrous or muscular systems can afford satisfactory interpretations of the phenomena of living beings; and on account of the disgust produced by the visionary properties and groundless hypotheses, originating in the humoural doctrines of GALEN. But late experiments have manifested, that various things taken into the stomach can be made at pleasure to produce considerable effects, by impregnating sensibly the blood and urine, as well as the milk, sweat, and perhaps saliva. Further; the fine experiments of Professor COLMAN have shewn, that the contagious glanders may be excited in the ass by the transfusion of the blood of a glandered horse, and the matter from the nose of the glandered ass can produce this disease in the horse or the ass.* Hence I apprehend it is reasonable to expect, that the farther investigation of the properties of the animal fluids will afford gratifying instruction to the Researcher in

* Mr. COLMAN alleges, that there is not a sufficient quantity of blood, in a single glandered ass, to excite the glanders by the transfusion of blood into the horse.

natural science, and important practical information to the Physician.

On the present occasion, I desire the honour of communicating the knowledge I may have acquired, by investigating the properties of expectorated matter secreted by the bronchial membrane. The appearances of this substance serve to regulate the judgment of the Physician concerning several diseases of the lungs; but especially of that of pulmonary tubercles which yearly destroys 120,000 to 140,000 subjects of the United Kingdom. It is fit that I remark, that I do not notice in this paper the ingenious experiments of several learned Chemists, because by so doing I should be led into a detail of too great extent for my design.

The numerous varieties of expectorated matter, according to my observation, may be arranged and characterized under the following seven heads:

I. The jelly-like semi-transparent kind of a bluish hue, excreted in the healthy state.

II. The thin mucilage-like transparent matter, so copiously expectorated in bronchial catarrhs.

III. The thick opaque straw coloured, or white and very tenacious matter, coughed up in a great variety of bronchial and pulmonary affections; especially in that of tubercles.

IV. Puriform matter secreted without any division of continuity, or breach of surface of the bronchial membrane, very commonly occurring in pulmonary consumptions.

V. The matter which consists of opaque viscid masses, together with transparent fluid; or the second sort above stated, with nodules of the third or fourth kind.

VI. Pus from the vomicæ of tubercles.

VII. Pus from vomicæ by simple inflammation of the lungs, and without tubercles.

Other kinds of matter are occasionally coughed up, such as calculi,—masses of self-coagulated lymph—serous fluid—blood itself,—and perhaps the vascular substance of the lungs; but I do not write on these matters, because they either do not belong to any particular recognized disease; or they are rare occurrences in some well known disease, and are too obvious to require description.

§ I. *Sensible, or obvious Properties.*

1. *The jelly-like matter*, as already said, is excreted in the best health, as well as sometimes in disease. It is mostly coughed, or hawked up, in a morning soon after a night's repose, during which it seems to accumulate. A few masses, or nodules, then appear of the consistence of jelly, and of the size of a pea to a hazle nut. It is also at any time liable to be excreted, in consequence of various extraneous matters irritating the fauces, to the amount of a few nodules. It is of a grayish colour or inclining to blue, with black specks; and it is rarely whitish in nodules. The consistence is that of jelly, but of much greater tenacity. It has a barely perceivable taste of common salt, or muriate of soda. It commonly floats on water, but by agitation to disengage air bubbles, it sinks. It has no smell. To the naked eye, or assisted by a single magnifier, this matter seldom appears uniform, but consists of a mixture of opaque and transparent masses of irregular figures. With the compound microscope, spherical

particles were perceived, though few in number, when duly diluted. The presence of an alkali I could in no instance perceive, by means of the usual tests, *namely*, turmeric paper, litmus paper slightly reddened by vinegar, and cloth stained with violet juice; nor was an acid denoted by means of litmus paper, except when I had reason to believe it was derived from various acid substances taken with the food, or drink, adhering to the inside of the mouth and fauces.

2. *The mucilage-like expectorated matter*, according to my observation, occurs much less frequently than the other sorts. It appears suddenly in great abundance in certain bronchial catarrhs. I have seen it to the amount of two, or three pints in twenty-four hours. It is also secreted, but less copiously in paroxysms of spasmodic asthma, and of the hooping cough; and but rarely in pneumonic, or pleuritic inflammations, and in some chronical organic diseases of the heart and lungs.

This matter is a transparent uniform fluid of the consistence of white of egg; or of a mucilage compounded of about one part of Arabic gum, and four or five parts of water. It is colourless,—has a fleshy smell—has a brackish taste. After standing eight or ten hours, a deposit takes place of fibrous, leaf-like, or curdy masses, some of which are seen suspended in the clear fluid. In some cases nodules of opaque thick ropy matter, at certain times, accompany this mucilage-like matter. Under the simple magnifier I perceived irregular figured masses partly in motion and partly suspended. With the microscope, globules were seen; but larger considerably than those of th blood, and much less numerous. With the usual tests there were no indications of alkali, nor of acid,

provided the matter was unmixed with other things. It usually floated, or was suspended in water, when first expectorated; but on standing in the water it fell to the bottom, evidently owing to the disengagement of air bubbles.

By standing exposed to the air in warm weather, it sooner grew fœtid than pus of abscesses; without becoming opaque. Neither could I render it opaque or thicker, by exposure to a stream of oxygen gas for an hour; or by exposure of it in a jar of this gas for a month.

8. *The opaque ropy matter* above-mentioned.

1st. It is secreted most copiously in that very common, and extensively epidemial disease of our climate, the *winter-cough*, occasioned by tubercles, to the amount of half a pint to a pint in twenty-four hours; especially during the winter season for several successive years, and sometimes during the whole of a long life, after the age of forty or fifty years. 2dly. It is often the expectorated matter of the pulmonary consumption of young persons, also occasioned by tubercles, but frequently mistaken for the pus of abscesses or vomicæ. 3dly. It appears, oftentimes, in pneumonic or bronchial inflammation with fever, seemingly being a beneficial discharge; as well as in some instances at the close of a fever without concomitant inflammation of the lungs. 4thly. A severe paroxysm of spasmodic asthma is often terminated in the excretion of this kind of matter. 5thly. A secreted substance of this sort is sometimes expectorated in various chronical organic diseases of the lungs, the heart, aorta, and parts contiguous to the lungs, which occasion difficult transmission of blood through them.

In all these instances the matter by expectoration is of the

consistence of thick cream, or of thin toasted cheese; so tough as to hang in the form of a rope, four or five inches in length, on pouring it from one vessel into another. Its aggregation is such that it is readily detached in large masses from the vitreous surface of vessels. It is not unusual for small black, or reddish spots, and streaks, to appear on the surface of this sort of expectorated substance. A pretty large bulk of it is seldom throughout uniform; but it is frothy, and exhibits opaque masses of various hues with transparent matter interposed. The *colour* is yellowish, straw-coloured, and white, or gray: it also, though seldom, is greenish and bluish. The *taste* asserted by patients, is, in their own terms, various, *namely*, saltish, nasty, faintish, sweetish, luscious, or like that of a sweet oyster,—a sharp or sour taste is the most rare. The only *smell* which I have perceived is that of flesh, but very frequently there is none. When any offensive or pungent smell was perceived, immediately after expectoration, I have always found that it was owing either to the foulness of the vessel in which it was received; or it was from extraneous matters in the mouth, and from decayed teeth.

This opaque viscid substance, being duly diluted with distilled water was examined with microscopes of common, as well as of very great powers: by means of any of them crowds of spherical particles were seen passing to and fro, in currents, not unlike those of the blood; except that they were larger. These globules I could not destroy, nor alter in form, by trituration; nor by long boiling in water; nor by exsiccation, and again dissolving in water; nor even by coagulation with mineral and vegetable acids, with alcohol, with sulphuric ether, or with tannin, and alum; nor by mixture with caustic

alkalies in a proportion which leaves the liquor turbid; nor for some time after the putrefactive process had appeared. But these globules disappear with such a proportion of sulphuric acid as detaches charcoal; or of nitric acid, and of liquid potash, as produce a clear solution: also by charring by fire. It is perhaps superfluous to remark, that these atomic globules are quite different from the air bubbles usually entangled in this kind of matter, as perceived by the microscope; the latter differ much from the former, in being of far greater magnitude—in being less numerous—in being transparent, and disappearing on agitation, or heating the matter, or even by mere standing.

For the most part this expectorated substance swims on water; but by agitation or stirring to disengage air bubbles, or by merely standing, it sinks. Some of the lumps suddenly hawked up, immediately fall to the bottom of a vessel of water. No signs of either acid, or alkali, appeared on the trials of this matter with well known reagents, provided it was free from extraneous matter; but it was apt to betray acidity from things taken with the food or drink.

4. *Puriform matter.* I have seen this matter expectorated in several diseases in the quantity of two or three ounces to half a pint in twenty-four hours, on some rare occasions, without any breach of surface. I believe it would be considered by every one to be *pus*, having the properties commonly admitted to be those of this substance. It will however, perhaps, only be just to call it *puriform*, for the present, as it appears to me probable, that I shall hereafter be able to show that it possesses properties not belonging to pus of abscesses, although in the obvious, or sensible properties, it is similar to

such pus. Accordingly this expectorated matter is not only opaque, white, or yellowish, and as thick as the richest cream, but it also has not more tenacity than cream. It is not apt to entangle air, and therefore it immediately mingles with water, rendering it milky; and presently subsides to the bottom, leaving the water clear, or at least whey-coloured. It appears to the naked eye uniform in its texture; and nearly so under the simple lens: but under the microscope thousands of globules similar to those of the blood are seen, which are indestructible as those above related belonging to another kind of expectorated matter.

The substance, of which I am now speaking, is most frequently excreted in the latter stages of pulmonary phthisis, for many weeks successively. It is taken for granted that this matter is from a breach of surface or ulceration; but on examination after death, such a state was not found, in many instances, under my observation, although the lungs were as usual full of tubercles and vomicæ. This puriform matter is occasionally expectorated in certain other diseases. The last summer my colleague, Dr. Nevinson, furnished me with several ounces of this sort of substance, but of a greenish hue, and of the consistence of thin cream; which was expectorated by a woman in the third week from the attack of the measles. In a few days she died. On examination of the lungs very carefully, by the excellent house surgeon of St. George's hospital, Mr. Dawes, no ulceration could be discovered in the trachea or in the bronchial tubes; nor were any tubercles, or abscesses found in the lungs. The patient, according to my information, had expectorated more than a pint of this fluid every twenty-four hours for a week before death.

In another hospital case, a man laboured under a cough with spitting of matter, which all who saw it called pus, and as usual it was considered to arise from an ulceration, or suppurated tubercles; but, on examination after death, the disease was ascertained to be condensation of the lungs, to the consistence of liver, with water in the cavities of the chest, and nothing more.

5. *Opaque viscid matter of the third, and perhaps fourth sort*, above distinguished, appearing in nodules, and irregular figured masses, *mixed with transparent slimy matter of the second sort.*

It is not unusual to see the mixture of these two different kinds, from severe fits of coughing in that constant epidemy of the British islands, the winter chronical pneumonia.

Different parts of the bronchial membrane being in different states, may account for the secretion of the two different matters. This seems more probable, than that these different matters should be secreted from the same part; although it is true that the same part does secrete at one period transparent thin slime, and at another an opaque thick matter. The former is occasioned by great irritation of the membrane, and the latter is the effect of a more gradual secretion with much less irritation.

For the sake of brevity, I avoid a further description. The practical application of these observations, however important, would not be suitable in this place.

The sixth and seventh kinds of expectorated substances being secreted after a quite different manner, and being very different in their nature from the preceding five kinds, I shall not give an account of them in this paper.

§ II. *Agency chiefly of Caloric.*

1. No effect of importance is produced by this agent, until the temperature of the expectorated matters is raised to about 150° of Farrenheit's thermometer: then the state of aggregation is evidently altered, the viscidity of each of them being diminished. At about 155°, coagulation begins to be quite evident in the first, third, fourth, and fifth kinds of matter—that is, curdy masses of various magnitudes appear in a milk-like, or whitish liquid. On elevating the temperature to 160° or 170°, a large proportion of curd is formed; but the proportion of the curdy matter to the liquid is very different in different specimens. The viscid texture, or tenacity of the expectorated matters, is by this treatment destroyed. The milky liquid decanted, after standing ten or twelve hours, affords, on evaporation to dryness, about three to four grains of residue from each 100 grains.

This liquid passes very slowly through the paper filter. The filtrated liquor affords scarcely more than one per cent. on evaporation to dryness. By repeatedly boiling in successive portions of water, the whole, as far as I could judge, of a given quantity of the curd might be diffused to form a whitish liquid; which on evaporation to dryness appeared to afford a residue of the same kind (except in containing a smaller proportion of saline substances), as the milky liquid which was separated from the curd on the coagulation of the expectorated matter.

The second kind, called *mucilage-like transparent matter*, does not afford curdy masses at the temperatures abovementioned; but its viscid texture is destroyed, and it becomes a whey-like, or somewhat milky liquid; and, on examination

with a magnifying glass, it appears full of curdy particles.—After this agency of caloric, the expectorated matter is much less prone to putrefaction.

2. Distillation of the expectorated matters to dryness afforded a perfectly limpid water, which had a peculiar smell, but no impregnation with ammonia; nor with any other substance which could be detected, except a little carbonic acid.

The residuary matter in a brittle state of dryness, afforded by evaporation, varied between two and a half and ten per cent. of the expectorated substances. The second kind yielded one thirty-fifth to one forty-fifth of its weight of brittle residue. The first kind afforded one twentieth to one twenty-fifth of residue. The third kind afforded very different proportions of solid residue, according to its consistence, *viz.* one tenth to one eighteenth of its weight. The fourth kind gave one twelfth to one fourteenth of brittle matter. The fifth kind yielded very different proportions of residue, according to the very different proportions of transparent and opaque matter, of which it consisted—it varied between one eighteenth and one thirtieth.

3. All these exsiccated substances on exposure to air, grew more or less moist, or at least were no longer brittle; but became somewhat soft, and, proportionately to the state of moisture, were augmented in weight. The thinner the expectorated matters, the moister and the greater increase of weight they generally experienced. But parcels of the same consistence from different patients sometimes differed much in degree of moisture, on exposure to the air. I have found some parcels of the second and fifth sorts of expectorated substances grow quite moist, and receive an increase in weight of three per cent. If the

residues were kept in close vessels, they remained in a brittle state. Larger parcels of exsiccated matter become more moist than smaller ones of the same kind in the same circumstances.

4. The milky and curdy liquids, which separated from the curdy masses (1) being poured off; and also the curdy masses being by pressure rendered dry; the liquids were evaporated to dryness, but became moist on exposure to the air. The curdy masses were by evaporation rendered brittle, and remained so in the air. The residues of the evaporated liquids were said to taste extremely salt, and the exsiccated curdy matter was tasteless.

5. The milky liquids (4) concentrated by evaporation, did not indicate any disengaged acid, nor alkali to the usual re-agents —By triturating these liquids with lime, a little ammonia was discharged—by trituration with concentrated sulphuric acid, the muriatic acid was disengaged—with phosphoric acid, and also with tartaric acid on trituration and heating, a pungent smell was perceived, somewhat like that of the acetous acid.—On burning to a brown ash the saline residue afforded by evaporation of these liquids, the predominating taste of it was that of muriate of soda. This ash readily melted,—being moistened, it turned turmeric paper to a reddish brown colour, and changed turnsole paper, reddened by acetous acid, to a deep blue—on exposure to the air, it partially deliquesced—the dissolution, by boiling in distilled water, afforded supertartrate of potash on the addition of the tartaric acid; and a red precipitate was occasioned by nitro-muriate of platina.* This incinerated and fused saline residue by other

* The knowledge of this re-agent, I believe, the chemical world owes to Dr. WOLLASTON.

trials, was proved to contain phosphoric acid, and lime; with traces of sulphuric acid, magnesia, iron, and perhaps silica; but the chief ingredients were muriate of soda, and potash.

6. The curdy matter after expression (4) afforded a much smaller proportion of brown ash than the fusible saline residue (5). It required an intense fire for fusion in a platina crucible. The fused mass did not deliquesce, but it grew somewhat moist on exposure to the air. It contained a much smaller proportion of potash than the former fused matter (5); also much less of muriate of soda, but a far larger proportion of lime and phosphoric acid with traces of sulphuric acid, magnesia, oxide of iron, and perhaps silica.

7. (a) 15400 grains of the third sort of expectorated matter on exsiccation, afforded 960 grains, that is, one sixteenth of brittle substance, or about six per cent., and of course this kind of matter contained about ninety-four per cent. of water (§ II. 2). This dried matter was reduced to a charred state by exposing it to fire in a Wedgwood white crucible. In this process it inflamed, emitted the usual smell of burning animal matter, especially of bone, and swelled prodigiously; at the same time a black oil was compounded rendering the mass soft during the inflammation. I could not distinguish the smell of sulphur, but there was, in one part of the burning, a smell, to my sense, of phosphorus.

(b). This charred matter was kept in a state of ignition in a platina crucible, till it no longer remained in a powdery form, but was reduced to a comparatively small bulk of a substance of the consistence of paste in an intense fire. By continuing the fire, the charge at length was fused; and after being kept in a state of fusion to be quite fluid for ten minutes,

the fire being withdrawn, a white, brittle, apparently saline matter, like melted common salt, was easily detached from the platina crucible, which in some places had received a red tinge.

(*c*). The melted matter (*b*) weighed fifty-nine grains; of course, this saline residue amounted to $\frac{1}{16}$ of the expectorated matter, and to one sixteenth of this expectorated matter exsiccated. It tasted only of muriate of soda—it had no smell—it effervesced with acids—it betrayed the presence of alkali to the tests above-mentioned—after a few days exposure to the air, it partially deliquesced—it precipitated supertartrate of potash with tartaric acid, and emitted no ammonia with lime; nor sulphur with muriatic acid discoverable by the most delicate tests.

(*d*). The fused matter (*c*) was boiled with three times its weight of distilled water, in which about one half appeared to dissolve. The clear liquid decanted from the sediment and evaporated, yielded crystals of muriate of soda with a much smaller quantity of spicula, or needle-shaped crystals; and saline matter which appeared under a lens not definitely crystallized. A second boiling of the sediment, with twice its quantity of water, afforded almost entirely muriate of soda. A third boiling gave a few crystals of this salt only, as appeared under the magnifier. A fourth boiling, in an equal weight of water, afforded no saline matter.

(*e*). The saline matters (*d*) amounted to forty-five grains when evaporated to dryness. I collected by means of a tooth-pick, from amongst the cubical crystals, as much as I could of the spicula and uncrystallized saline matter. These parts effervesced and precipitated supertartrate of potash with tartaric

acid, and certainly afforded no soda-tartrate of potash—they also afforded a precipitate with nitro-muriate of platina—being saturated with acetous acid there was still a slight precipitation with muriate of baryt; for without acetous acid, there was a more copious precipitation with this re-agent, but the greater part of the precipitate was dissolved by acetous acid, added so as not to supersaturate it.—Oxalate of ammonia did not occasion a precipitation.—With nitrate of silver an abundant curdy took place.—lime water produced only slight turbidity. The muriate of soda amounted, in this saline mass of forty-five grains, to thirty-five grains, or nearly to one grain in 450 of expectorated matter; the rest was subcarbonate of potash amounting to one grain in about 1440 grains of expectorated matter, with which was mixed a minute proportion, probably, of sulphate and of phosphate of potash.

(*f*) The undissolved matter (*e*) boiled with muriatic acid gave a turbid liquid, but on standing, nearly the whole appeared to have been dissolved: a small proportion of sediment only took place in a transparent liquid, which was boiled till it no longer parted with muriatic acid.—This dissolution being evaporated grew liquid on exposure to air; and oxalate of ammonia gradually added, produced, as I decidedly ascertained, the precipitate of oxalate of lime.

(*g*). The filtrated residuary liquid (*f*); with muriate of baryt gave immediately a copious precipitation.—With lime water there was milkiness produced, and subsequently a white precipitation which did not disappear on adding a small proportion of acetous acid.—prussiate of potash occasioned a greenish blue colour without precipitation.—succinate of am-

monia produced a milky liquid—no effect was observed from tartaric acid.—There being a precipitation with caustic or pure ammonia, as well as with potash, and with the carbonates of the alkalies, it was supposed magnesia was present: and the dissolution of this precipitate in muriatic acid, and in acetous acid, gave no precipitate with oxalic acid. Some of the muriatic dissolution, previously to precipitation with oxalate of ammonia (*f*), being evaporated to dryness, the residue was ignited; but if magnesia was present, as well as lime, it was in too small quantity to be distinguishable from the lime, by composing sulphate. The precipitate now under examination, was certainly not mere magnesia, for it melted into an opaque globule under the blow-pipe—it was not phosphate of lime, for with sulphuric acid, a somewhat bitter and sour substance was compounded, which afforded a precipitate with ammonia, but none with oxalate of ammonia. It was a phosphate not only on account of its fusibility, but because a curdy appearance was occasioned by the mixture just mentioned, with sulphuric acid, on adding it to lime water. Neither was it soluble, like phosphate of lime, in phosphoric acid. The quantity of this precipitate was too minute for decisive experiments, but from those related, it seems probable that it was phosphate of magnesia, which was dissolved, as will appear presently, in phosphoric acid, and precipitated by ammonia.

(*h*). The residuary liquid (*g*), after the precipitation by oxalate of ammonia, being evaporated to dryness, was easily ascertained to be phosphate of ammonia, with indications of a minute proportion of sulphate.

(*i*). It remains only to notice the indissoluble matter in

muriatic acid (*f*). I found it to grow soft, and the parts to cohere under the blow-pipe, and with a little potash it readily melted into an opaque globule.

8. To obtain a more satisfactory proof of the presence of sulphur, forty grains of charred expectorated matter were kept in a state of ignition in a platina crucible, with another inverted over it to completely exclude the escape of gas, for two hours. After cooling, the smell of sulphuretted hydrogen gas very evident, on the addition of diluted muriatic acid, and even of water only. Silver was tarnished, and paper wetted with liquid acetite of lead was blackened by this gas. In some of the experiments, while the charcoal was burning off from the charred expectorated matter, I perceived the smell of sulphur, and perhaps of phosphorus.

§ III. *Agency of Alcohol of Wine.*

1. (*a*) 2500 grains of desiccated expectorated matter of the fifth sort, § I. 5 being the one twentieth of 50,000 grains of matter previously to evaporation to dryness, were digested in four pints of alcohol of spirit of wine, of the specific gravity of 815, water being 1000. The mixture was exposed at the temperature of 58° to 68° for a month, during which it was frequently shaken. A tincture, of the colour of red port wine, was then decanted from off a blackish sediment. By means of a press, two ounces more of the tincture were obtained.

(*b*) The undissolved residuary matter being exsiccated weighed 130 grains less than before digestion. On exposure to the air, it remained dry, but it became more flexible. It no longer emitted ammonia on trituration with lime.

(*c*). The tincture thus obtained was distilled readily till there remained about five ounces measure in the retort, and what remained seemed to be chiefly water instead of spirit, with such a quantity of matter dissolved in it, as not to afford liquid by distillation, without frequently spirting into the receiver. The residuary liquid was therefore evaporated to the consistence of a soft resin-like extract of a black colour; which had a salt with bitter taste.

The distilled liquid had a peculiar pungent smell, but not that of ammonia, and it neither reddened turnsole paper, nor rendered violet cloth green.

(*d*). The resin-like extract (*c*) weighed 140 grains. It was semi-transparent—dissoluble in water, but not coagulable in boiling water—it grew softer on exposure to air—it was uncrystallizable—it betrayed no signs of alkalescency nor of acidity, except just giving turnsole paper a reddish hue—under the blow-pipe it burnt like matter from animals, and afforded a fused globule, which indicated muriate of soda, and a large proportion of potash, deliquescing very speedily—with lime, it emitted the smell of ammonia—with phosphoric, and also with tartaric acid, on being heated, an acid smell was perceived which I at first mistook for acetous acid, for I soon found that no such acid was present, not being able to detect a trace of any acid in the distilled liquid from these mixtures—on the addition of acetite of lead, a very copious precipitation of fawn-coloured sediment instantly took place, with the smell most distinctly of apples. The decanted liquid of this mixture was found to be chiefly acetite of potash. On dropping diluted sulphuric acid upon the fawn-coloured sediment, it constantly emitted the smell of apples. I could not,

however, satisfy myself, that the small quantity of liquid decanted from off this sediment contained a kind of vegetable acid for the first time apprehended in the fluids of animals; because, first, the quantity of product I possessed was so diminished by many experiments, that I was unable to make what I considered to be decisive trials. Secondly, because in subsequent processes I failed in producing the same apple-smelling liquid. Hence I considered that the supposed acid, which had some of the properties of the malic, only occurred occasionally, or that I had been deceived, and that I had procured nothing more than a little of the acid employed for the decomposition, disguised by mixture with the subject of the experiments. The fawn-coloured precipitate was, no doubt, chiefly muriate of lead. Still the experiments fully demonstrate the presence of potash neutralized, either by an acid destructible by fire and dissoluble in alcohol, but hitherto not disunited from animal oxide, or that an oxide of animal matter alone neutralizes the potash, as will be manifested by the evidence of experiments to be related.

(*e*). Forty-five grains of the residue (*c*) which had been dissolved in alcohol, being burned in a platina crucible, yielded chiefly potash, and half its quantity of muriate of soda.

(*f*). Twenty-five grains of the residue (*c*) were boiled with successive portions of nitric acid, till the oxide of animal matter was decompounded and carried off in the state of gases; and then deflagration took place, leaving subcarbonate of potash with muriate of soda and charcoal.

According to a computation, the 140 grains of resin-like extract (*c*, *d*) consisted of twenty-eight grains of potash, and

eighteen grains of muriate of soda, with an inappreciable quantity of ammonia, and perhaps phosphoric acid, besides the oxide of animal matter, and possibly an acid of an unknown kind.

(*f*). The undissolved matter (*b*) was burned in a platina crucible. It afforded a residue, which I could not render fluid by fire, but only of the consistence of paste. On cooling, it was a brittle gray mass weighing fifty-six grains, somewhat salt and gritty to the taste. It consisted of muriate of soda and phosphate of lime, about twenty-three grains of each, — of potash four grains, — of fused matter, which by long boiling in muriatic acid yielded phosphate of lime, muriate of lime, and utterly indissoluble vitrefied matter with traces of magnesia, oxide of iron, and a sulphate.

2. Four thousand grains of expectorated matter of the third kind, page 317, § II. 3 were added to two pints of rectified spirit of wine. By agitation, the spirit became at first milky, but presently it grew clear; little curdy masses appearing, which fell to the bottom as a sediment, being in bulk about one fourth of that of the added expectorated matter.

After a month's digestion, the filtrated liquid, on evaporation, afforded a dry extract-like residue, weighing sixty grains. It grew moist by exposure to the air, but not when kept in close vessels. It consisted of the same ingredients, but in very different proportions, as the residue from distilling and evaporating the tincture, page 323, § III. 2, the present residue containing a much larger proportion of muriate of soda, and oxide of animal matter.

Successive digestions of the same matter afforded less and less saline residue, but nearly the same proportion of oxide

of animal matter for three times, but then no saline matter was afforded, but merely animal matter. The residues of the evaporated tinctures of the subsequent digestions did not, like the first, grow moist, but only softer; and the oxide of animal matter from each of them was no longer coagulable, although afforded by dissolution of coagulated matter. It appeared that the animal oxide was of one kind only, and that the whole of it might be dissolved in alcohol, and thereby become uncoagulable, and more easily dissoluble in every kind of menstruum.

3. If a large proportion, namely, two parts of expectorated matter be mixed with two parts of rectified spirit of wine, the matter is in great part, at least, coagulated, but the spirit is rendered milky. The same is true with regard to other menstrua. The reason is obvious. The coagulation is produced by the separation of water from the animal oxide of the expectorated matter, by the attraction of the alcohol, or of acetous acid for the water; but if there is not a due proportion of spirit or acid, the oxide of animal matter retains so much of the water, as to render the liquid milky. A person accustomed to these experiments may determine pretty exactly by means of them, the proportion of water in the expectorated matter, it being directly as the quantity of spirit or acid requisite to produce entire coagulation in a clear liquid; and the proportion of coagulable animal oxide is, within certain limits, inversely as the quantity of spirit requisite for coagulation.

4. Sulphuric ether, being in many properties analogous to alcohol of wine, I digested three hundred grains of exsiccated matter of the third kind, page 317, in four ounces measure of

this menstruum for a month, in a warm room, during which the vessel was often agitated. Three ounces of a black tincture were thus procured, which, on distillation to dryness, afforded sixty-five grains of soft extract. This extract became a little moist on exposure to the air, and was then a little viscid. It burnt with flame like oil to the state of charcoal; and this again on burning, only left two grains of residue, which consisted of muriate of soda, with indications of alkali, and phosphate of lime.

The undissolved residue also remained soft, and could not be made brittle by evaporation. After inflammation and incineration, the usual products were obtained as from matter which had not been digested. This menstruum had therefore dissolved abundantly the oxide of animal matter, and but a small proportion of the saline and earthy parts.

4. Apparently uniform expectorated matter is not of the same consistence through the whole mass; for a few drops of the opaque kind being shaken in half a pint of rectified spirit of wine, the whole does not dissolve, but it is broken into small curdy particles, which fall as a sediment in a clear liquid, seemingly about one-fourth of the original bulk of the matter.

§ IV. *With Water.*

1. None of the kinds of expectorated matter are readily diffusible through cold water, except the second and fourth, page 316 and 319; and by agitating them some fibrous pieces are usually detached; also on inspecting the water after this diffusion, it appears full of small masses, or motes. On standing, these suspended masses become a sediment; which is the

case, although the proportion of expectorated matter be exceedingly small to that of the water.

2. When very hot water is used, namely, that of the temperature of 190° to 210°, a still greater number of motes are perceivable, especially with a lens, and the water is rendered milky.

3. Brisk agitation is required, for a due length of time, to diffuse the other kinds of expectorated matter through cold water; but a great number of fibrous and membranous pieces appear, whose form cannot be destroyed or only partially, by shaking, in almost any proportion of water. Three drops of ropy and opaque matter were shaken in half a pint of distilled water. About one half of them was diffused; the rest was in the form of small fibrous, leafy, and irregular figured motes; which, on repose, formed a sediment, and remained in that state three months; although in that time the water became highly fœtid, and sometimes in this experiment the sides of the vessel were tinged black.

4. Agitation of these sorts of expectorated matter (3), in a large proportion of water at the temperature of 170° and upwards, produced a greater degree of milkiness, and a greater number of small masses, which could not be dissolved by long shaking. Putrefaction did not take place so soon in these mixtures, as in those with cold water.

5. If the proportion of the last mentioned kinds of expectorated matter be two or three parts to one of cold water, or under the temperature of coagulation, an uniform mixture may be produced by violent agitation, the water being entangled by the viscidity of the matter rather than chemically united.

6. On boiling the mixtures (5), a great part of the expectorated matter is separated in a curdy form, from a milky liquid.

7. If less than two grains of expectorated matter were diffused through five hundred grains of water, no evident precipitation was occasioned by tannin; while with one grain of isinglass jelly, or white of egg, or of serum of blood dissolved in five hundred grains of water, there was an evident precipitation with this re-agent.

8. I could arrive at no useful conclusions, for the distinction of expectorated matter from other coagulable, or from any gelatinous substances by comparative trials with muriate of tin, nitro-muriate of gold, oxymuriate of mercury, acetite of ceruss, and acetite of litharge.

§ V. *Agency of acetous Acid.*

1. Twenty ounces of ropy opaque matter, by being shaken with ten pints of distilled vinegar, were so broken into a fibrous or even vascular form as to exhibit an organized appearance, the bulk being reduced to at least one third of the ropy matter. By repeated agitation and long digestion, the coagulated masses were broken into smaller pieces, but did not appear to be further contracted in bulk, or to dissolve. With some parcels of matter, the vinegar preserved its transparency, with others it became whey-like, the matters being deposited in a curdy state. The mucilage-like expectorated matter, or this mixed with the other kinds, afforded whey-like, or more or less turbid liquids with vinegar.

2. (*a*) The decanted liquid, and the liquid obtained by pres-

sure of the sediments of the last mixture (1), being distilled to about one eighth, the remainder was evaporated to the consistence of a thick extract. The distilled liquid did not appear to have received any impregnation, except what had altered a little the odour. This extract-like residue amounted to one forty-fifth to one eightieth the weight of the expectorated matter, according to the kind of this substance. It varied also according to the proportion of the matter to the acid menstruum.

(*b*). The residue (2, *a*) just mentioned, after digestion a second time, in the same quantity of acid, afforded a smaller quantity of extract-like matter than before.

(*c*). The third digestion afforded still less of this substance.

(*d*). The fourth and fifth digestion gave somewhat less than the immediately preceding one.

(*e*). The sixth digestion yielded nearly the same proportion of extract-like matter as the fourth and fifth.

3. The undissolved matter, after these repeated digestions in vinegar (1, 2), being exposed to fire in a platina crucible, first flamed and partially melted; then became apparently charcoal, which burned away to the state of a brown earth-like substance scarcely $\frac{1}{160}$ of the weight of the substance subjected to fire, and not above $\frac{1}{1600}$ of the expectorated matter by which it was afforded. It consisted chiefly of phosphate of lime, with indications of carbonate of lime, of a sulphate, of a muriate, of silica, or at least vitrified matter, and of oxide of iron.

4. The extract-like matter, from the first digestion of the expectorated matter (2 *a*), by exposure to the air, in a few

days partially deliquesced, affording no signs of alkalescency, but having a peculiar salt taste.

(*a*). A little of this deliquescing part being boiled to dryness, with a large proportion of nitrous acid, on beginning to be ignited, it deflagrated, leaving a blackish saline residue; which soon deliquesced, and being lixiviated, it precipitated supertartrate of potash with tartaric acid, and gave a reddish precipitate with nitro-muriate of platina. The residue also contained lime, for the dissolution in acetous acid afforded oxalate of lime, on the addition of oxalate of ammonia.

(*b*). This extract-like matter (a *a*), by digestion in rectified spirit of wine, gave a blackish tincture, which being decanted and evaporated, left a residue. This became quite liquid after twenty-four hours exposure to the air. It consisted chiefly of acetite of potash, with an inappreciable portion of muriate of soda, and ammonia neutralized, probably, by phosphoric acid; besides uncoagulable and ungelatinizable oxide of animal substance.

(*c*). The undissolved matter by spirit of wine, just spoken of (*b*), after expression, being desiccated, it remained in a solid state after exposure to the air, only growing a little soft in four weeks time. By combustion, it afforded a difficultly fusible ash, which after fusion was found to consist chiefly of phosphate of lime, muriate of soda, with a little potash; a sulphate, traces of iron, and vitrified matter, which probably contained silica united to the other substances manifested in this fused mass.

5. The extract-like matter, by acetous acid on the second digestion (a *b*), grew soft, but did not deliquesce on exposure to the air. It was found to differ from the matter obtained

by the first digestion in the same menstruum, in containing a much smaller proportion of potash and muriate of soda, as well as of neutralized ammonia.

6. The extract-like matter, from the third digestion in vinegar (2 *c*), differed from the former, in containing a still much less quantity of the salts just mentioned.

7. The fourth and subsequent digestions (2, *d*, *e*) afforded extract-like substances, which contained scarcely any thing but a very small proportion of earthy phosphates, and indissoluble vitrified matter, produced by incineration and fusion. It did not appear that the oxide of animal matter, dissolved by the distilled vinegar in all the preceding digestions successively, was of different kinds; but it appeared, that its coagulable property was destroyed by dissolution in this menstruum. Accordingly, there is no reason to believe that the whole of this oxide is not dissoluble in the acid here employed, although the requisite proportion may decrease after each digestion, within certain limits.

8. A few drops of opaque ropy matter being agitated in half a pint of vinegar, a number of fibrous masses appear, apparently one fourth or one fifth the bulk of the matter added; and these fibrous forms subsist, notwithstanding continued agitation, totally disappearing only in consequence of long digestion in successive large quantities of this acid.

§ VI. *Some Experiments with different Objects.*

1. To produce a synthetic proof that potash may be neutralized by oxide of animal matter, I triturated ten grains of the exsiccated and coagulated part of expectorated matter freed from all saline substance, with pure potash gradually added,

and a little water. Several grains were in this way united, without any effect being produced by the compound on turmeric paper. More alkali was added till the compound barely manifested the existence of alkali to the test just mentioned. It was then digested in spirit of wine, to which it imparted a deep brown colour, and the tincture being distilled, it afforded a dry extract, which grew moist on exposure to the air, but scarcely affected turmeric paper. On incineration, however, the alkali was denuded, and fusion was produced easily.

An equal portion of the animal oxide, of the same parcel as in the last experiment, was digested in spirit of wine, in the same circumstances as this oxide united to potash. It imparted no colour to the spirit, and the extract obtained was in smaller quantity, than in the preceding experiment. Being evaporated to dryness, the residue did not grow moist, but it became a little soft on exposure to the air. Being exposed to fire, it left an inconsiderable proportion of infusible residue, with barely traces of alkali and muriate.

2. To determine, by a more satisfactory experiment than a preceding one, whether or not acid was united to the potash and evaporable, ten ounces of watery liquid, which separates from the curd on boiling expectorated matter, were evaporated to the consistence of a thin extract. This matter indicated neither acid nor alkali in a disengaged state, but it was ascertained to contain a large proportion of potash combined; and an acid smell was perceived on heating it with phosphoric or tartaric acid. Ten drops of liquid phosphoric acid were mixed with four hundred grains of this extract-like matter, and at a low temperature it was subjected to distillation to become almost a dry substance, but no acid could be detected

in the little liquid which came over, nor did this dry substance indicate any acidity to the usual re-agents—neither on exposure to the air did it, as before the addition of acid, grow moist. Phosphoric acid was further added till it became sensible to the test of turnsole: but neither by elutriation nor distillation could any acid be obtained, except a small portion of the phosphoric acid by elutriation, the rest having united to the potash.

3. To furnish an estimate of the proportion of ammonia, I subjected to distillation, a mixture of a pint of expectorated matter of the fifth kind, *page* 321, with three ounces of well burnt lime, but I could not reckon the ammonia in the distilled liquid at more than two cubic inches, or less than half a grain in weight.

§ VII. *Conclusions.*

1. From the preceding experiments and observations, and from others which I might have related, it does not appear that the various kinds of expectorated matter, *page* 314, differ in the ingredients of their composition, but merely in the proportion of them to one another.

2. It has been shown that expectorated matter consists of coagulable, or, as it is also now frequently termed *albuminous* animal subtance, and of water impregnated with several saline and earthy bodies—that the largest proportion of the animal substance, which may justly be called an oxide, amounts to one twelfth, and in some very rare cases to one tenth of the expectorated matter, reduced to a brittle state by evaporation; and that the smallest proportion of this oxide, in rare instances, amounts to one forty-fifth of the expectorated matter; but that the usual proportions of it vary between one twentieth and one sixteenth of this coagulable oxide to the evaporable

water, that is, between five and six per cent. of the expectorated matter.

3. The impregnating substances have been shown to be Muriate of soda, varying commonly between one and a half to two and a half per 1000 of the expectorated matter—Potash varying between one half and three fourths of a part per 1000—Phosphate of lime about half a part of 1000—Ammonia, united probably to the phosphoric acid; Phosphate, perhaps of magnesia; Carbonate of lime; a Sulphate; vitrifiable matter, or perhaps silica; and oxide of Iron. But the whole of these last six substances scarcely amounting to one part in 1000 of the expectorated matter, it would be useless to estimate the proportion of each of them. It is very probable that the proportions and quantities of these ingredients, vary much more than now represented in different states of disease and health.* It is very probable also, that some of the ingredients may occasionally be absent, and others of a different kind be present, agreeably to the different states, on different occasions of the other secretions.

4. It is manifest that the different states of consistence of expectorated matter, are owing to the proportion of albuminous or coagulable oxide, but I purposely avoid giving an account of the different conditions of health, on which the differences of consistence depend.

5. The thicker the matter, the smaller I commonly found the quantity of saline impregnation. Hence, in sudden and copious secretions of the bronchial membrane, the matter is asserted to be salt, and to feel hot. In such instances, the proportion of coagulable matter was small, but that of the

* In one case, the opaque expectorated matter in a pulmonary consumption having been exsiccated to brittleness, became almost liquid after a night's exposure to the air.

saline impregnations, particularly of the muriate of soda, and neutralized potash so great, that the exsiccated expectorated substance tasted very salt, and presently grew moist, or even partially deliqueseed; but the opaque ropy or puriform matter afforded a much larger proportion of exsiccated residue, which was but slightly salt, and generally only became soft on exposure to the air. This property of growing moist depends upon the potash.

6. Each of the human fluids, according to my experiments, contain neutralized potash; at least, this is the fact of the blood, dropsy fluid, pus of abscesses, and pus secreted without breach of surface; the fluid effused by vesicating with cantharides; the urine; and in course in the very abundant secretion from the nose by a catarrh. The alkali being united to oxide of animal matter in these fluids, it is easily demonstrable.

7. Although I think I have discovered many properties by which expectorated secretion may be distinguished from expectorated pus, I shall not speak of them, on this occasion, further than just to observe that the saline impregnation of pus, particularly that of potash, and muriate of soda is in very much less proportion than in expectorated secretion; and hence it does not become moist after exsiccation, on exposure to the air.

8. It has been, I believe, uniformly asserted, that the circulating and secreted fluids are impregnated with soda; that it is especially in the matter secreted by the bronchial membrane. The experiments of others must confirm or disprove mine. It seems, however, much more reasonable, that the human fluids should be found to contain potash than soda, united to some oxide or destructible acid; because the former alkali is daily introduced with the vegetable food, and with the

drink of fermented liquor; and it is as little likely to be destroyed, as the muriate of soda also induced in the very same way. But our food and drink do not, commonly at least, contain the soda united to a destructible acid, or an oxide.

9. It is plain, from the preceding experiments, that expectorated matter belongs to the class of coagulable fluids, and not of gelatinizable, or, as commonly asserted, mucous fluids. It differs from the coagulable fluid, serum of blood, in forming a much thicker fluid with a much larger proportion of water: for serum and also the water of blisters, is quite liquid, although they afford, on exsiccation, one twelfth to one eleventh of their weight of brittle residue, while some kinds of expectorated matter, of the consistence of mucilage, afford only one fortieth of dry residue, and others of the consistence of thin paste, afford only one fourteenth of residue.

10. But for the unavoidable extent of this paper, I should trouble the learned Society with various other conclusions and remarks, especially concerning the *globularity* of expectorated matter, which seems to indicate organization. Although Antonius Van Lewenhoeck, above a century ago, discovered the globularity of the blood, and even noticed it in other animal fluids, neither he, nor any other person, as far as I know, investigated the subject in any fluid but the blood, till by Mr. Home's acuteness and industry, at a very early period of life, it was observed in pus. I have in this paper related, that expectorated matter, especially the opaque ropy kind, as well as the puriform, is full of globules, and that, except by such agents as destroy charcoal, they are scarcely destructible. Do these spherical particles consist chiefly of organized carbonaceous matter?

XX. *On the Attractions of homogeneous Ellipsoids.* *By* James Ivory, *A. M.* *Communicated by* Henry Brougham, *Esq. F. R. S.*

Read June 15, 1809.

1. The theory of the figures of the planets involves in it two distinct researches. In the first of these, it is required to determine the force with which a body, of a given figure and density, would attract a particle of matter, occupying any proposed situation: in the second, the subject of investigation is the figure itself, which a mass of matter, wholly or partly fluid, would assume, by the joint effect of the mutual attraction of its particles, and a centrifugal force arising from a rotatory motion about an axis. To render the second of these inquiries more exactly conformable to what actually takes place in nature, the influence of the attractions of the several bodies, that compose the planetary system, ought to be superadded to the forces already mentioned.

It is the first of these two researches, of which we propose to treat at present; and we shall even confine our attention to homogeneous bodies, bounded by finite surfaces of the second order.

The theory of the attractions of spherical bodies is delivered by Sir Isaac Newton in the first book of the Principia.* In the same place the illustrious author lays down a method for

* Sect. 12.

determining the attractions of round bodies (or such as are generated by the revolving of a curve about a right line which remains fixed) when the attracted point is situated in the common axis of the circular sections:* and he employs this method to compute the attractive force of a spheroid of revolution on a point placed in the axis.† MACLAURIN was the first who determined the attractions of such a spheroid generally, for any point placed in the surface, or within the solid. The method of investigation, invented by that excellent geometer, is synthetical, but original, simple, and elegant, and has always been admired by mathematicians. When the attracted point is placed without the solid, the difficulty of solving the problem is greatly increased; and it was reserved for LE GENDRE to complete the theory of attractions of spheroids of revolution, by extending to all points, whether without or within the solid, what had before been investigated for the latter case only.‡ LA PLACE took a more enlarged view of the problem; he extended his researches to all elliptical spheroids, or such solids whose three principal sections are all ellipses; and he obtained conclusions with regard to them, similar to what MACLAURIN and LE GENDRE had before demonstrated of spheroids of revolution. In this more general view of the problem, the investigation is particularly difficult, when the attracted point is placed without the solid. The method of investigation, which LA PLACE has employed for surmounting the difficulties of this last case, although it is entitled to every praise for its ingenuity, and the mathematical skill which it displays, is certainly neither so simple nor so direct, as to

* Sect. 13, Prop. 91. † Prop. 92. Car. 2.

‡ *Acad. des Sciences de Paris, Savans Etrangers*, Tom. X.

leave no room for perfecting the theory of the attractions of ellipsoids in both these respects. It consists in shewing that the expressions for the attractions of an ellipsoid, on any external point, may be resolved into two factors; of which, one is the mass of the ellipsoid, and the other involves only the excentricities of the solid and the co-ordinates of the attracted point: whence it follows, that two ellipsoids, which have the same excentricities, and their principal sections in the same planes, will attract the same external point with forces proportional to the masses of the solids. This theorem includes the extreme case, when the surface of one of the solids passes through the attracted point: and by this means the attraction of an ellipsoid, upon a point placed without it, is made to depend upon the attraction which another ellipsoid, having the same excentricities as the former, exerts upon a point placed in the surface.* Le Gendre has given a direct demonstration of the theorem of La Place, by integrating the fluxional expressions of the attractive forces; a work of no small difficulty, and which is not accomplished without complicated calculations.† In the *Mecanique Celeste*, the subject of attractions of ellipsoids is treated by La Place after the method first given by himself in the Memoirs of the Academy of Sciences,‡ founded on the theory of series and partial fluxions. It was in the study of La Place's work, that the method I am about to deliver, was suggested; and it will not be altogether unworthy of the notice of the Royal Society, if it contribute to simplify a branch of physical astronomy of great difficulty, and which has so much engaged the attention of the most eminent mathematicians.

* *Acad. des Sciences de Paris pour* 1783. † Ibid. 1788. ‡ For 1783.

2. Let a, b, c, be three co-ordinates, that determine the position of a point attracted by a solid: and let dM denote a molecule, or element of the mass of the solid, whose position is fixed by the co-ordinates x, y, z, respectively parallel to a, b, c: then, supposing the invariable density to be denoted by unity, if we put $f = \{(a-x)^2 + (b-y)^2 + (c-z)^2\}^{\frac{1}{2}}$ the distance of the molecule from the attracted point, the direct attraction of the molecule on the point will be $= \frac{dM}{f^2}$. This force of attraction is next to be decomposed into other forces, having fixed directions independent on the position of the attracting molecule; and the directions most naturally suggested for this purpose, are the three axes respectively parallel to the co-ordinates. When the direct attraction is thus decomposed, the resulting forces, acting parallel to the axes, and directed to the planes from which the co-ordinates are reckoned, will be respectively,

$\frac{dM(a-x)}{f^3}$, parallel to the axis of x,

$\frac{dM(b-y)}{f^3}$, parallel to the axis of y,

$\frac{dM(c-z)}{f^3}$, parallel to the axis of z.

Let A denote the accumulated amount of all the attractions, parallel to the axis of x; and, in like manner, let B and C denote the same things for the attractions parallel to the axes of y and z: then, by restoring the value of f, and writing $dx \cdot dy \cdot dz$ for its equivalent dM, there will be obtained,

$$A = \iiint \frac{dx \cdot dy \cdot dz \cdot (a-x)}{\left\{(a-x)^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{3}{2}}}$$

$$B = \iiint \frac{dx \cdot dy \cdot dz \cdot (b-y)}{\left\{(a-x)^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{3}{2}}} \quad (1)$$

$$C = \iiint \frac{dx \cdot dy \cdot dz \cdot (c-z)}{\left\{(a-x)^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{3}{2}}}$$

where the several triple fluents must be extended to all the molecules that compose the mass of the solid.*

The expressions of A, B, and C, just found, are all integrable with respect to one of the variable quantities they contain. Thus A is integrable with respect to x. Let x' be the greatest value of x (y and z remaining constant) on the positive side of the plane of y and z, and x'' the greatest value, on the negative side of the same plane; then, the integration being performed, we shall get

$$A = \iint dy \cdot dz \cdot \left\{ \frac{1}{\left\{(a-x')^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{1}{2}}} - \frac{1}{\left\{(a+x'')^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{1}{2}}} \right\}.$$

In this expression of A, the fluxion under the sign of double integration denotes the attraction which a prism of the matter of the solid, whose length is $x' + x''$ and its base $dy \cdot dz$, exerts on the attracted point, in the direction of the length of the prism.

If the plane, to which x is perpendicular, bisect the solid, as is the case of the principal sections of solids bounded by finite surfaces of the second order, then $x' = x''$; and as x' is nothing more than what x becomes at the surface of the solid, if we now suppose x, y, z to be three co-ordinates of a point in the surface, and, for the sake of brevity, put

* *Mecan. Celeste*, Tom. I. p. 3.

$$\Delta = \left\{(a-x)^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{1}{2}}$$
$$\Delta' = \left\{(a+x)^2 + (b-y)^2 + (c-z)^2\right\}^{\frac{1}{2}}$$

then,

$$A = \iint dy\,.\,dx\,.\left\{\frac{1}{\Delta} - \frac{1}{\Delta'}\right\} \quad (2)$$

this double fluent is to be extended to all the points, or indefinitely small spaces $dy\,.\,dx$, that compose the principal section of the solid made by the plane of y and z.

In like manner, if B and C be integrated; the first with respect to the variable y, and the second with respect to the variable z; two new expressions of these attractions will be obtained, exactly similar to the expression for A, that has just been investigated.

3. The general equation of a surface of the second order bounding a finite solid, is*

$$\frac{x^2}{k^2} + \frac{y^2}{k'^2} + \frac{z^2}{k''^2} = 1:$$

if the three quantities k, k', k'' be supposed to be all equal, then the solid will be a sphere; if two of them, as k' and k'' be equal, it will be a solid of revolution; and if all the three be unequal, it will be an ellipsoid, or a spheroid, having all its three principal sections ellipses. In what follows, we shall always suppose that k is the least of the three quantities k, k', k'', or the least of the semi-axes of the solid.

The general equation of the ellipsoid, will be satisfied by putting $x = k \cos.\,\varphi$, $y = k' \sin.\,\varphi \cos.\,\psi$, and $z = k'' \sin.\,\varphi \sin.\,\psi$; where φ and ψ denote two indeterminate angles. In order to substitute these values of x, y, and z in the formula (2), we must begin with taking the fluxion of y, on the sup-

* *Mecan. Celest.* Tom. II. p. 7.

position that one of the indeterminate angles is constant; thus, if ψ be constant, then $dy = k \cos.\varphi \cos.\psi \cdot d\varphi$: and, because y must be constant when x varies, we must make

$$dx = k'' \cos.\varphi \sin.\psi \cdot d\varphi + k'' \sin.\varphi \cos.\psi \cdot d\psi$$
$$0 = k' \cos.\varphi \cos.\psi \cdot d\varphi - k' \sin.\varphi \sin.\psi \cdot d\psi,$$

and, by exterminating $d\varphi$, we get $dx = \frac{k'' \sin.\varphi}{\cos.\psi} \cdot d\psi$. Thus, by substitution, the formula (2) will become

$$A = k' k'' \iint \sin.\varphi \cos.\psi \cdot d\varphi \cdot d\psi \left\{ \frac{1}{\Delta} - \frac{1}{\Delta'} \right\}; \quad (3)$$

and,

$$\Delta = \left\{ (a - k \cos.\varphi)^2 + (b - k' \sin.\varphi \cos.\psi)^2 + (c - k'' \sin.\varphi \sin.\psi)^2 \right\}^{\frac{1}{2}}$$
$$\Delta' = \left\{ (a + k \cos.\varphi)^2 + (b - k' \sin.\varphi \cos.\psi)^2 + (c - k'' \sin.\varphi \sin.\psi)^2 \right\}^{\frac{1}{2}}:$$

the double fluent must be taken from $\varphi = o$, to $\varphi = \frac{\varpi}{2}$ (ϖ denoting half the pheriphery of the circle, whose radius is 1), and from $\psi = o$, to $\psi = 2\varpi$.

To obtain a further transformation of the last expression of A, we are now to determine the semi-axes of an ellipsoid, whose surface shall pass through the attracted point, and which shall have the same excentricities, and its principal sections in the same planes, as the given ellipsoid. Let h, h', h'' be the semi-axes required: then, because the attracted point is to be in the surface of the solid,

$$\frac{a^2}{h^2} + \frac{b^2}{h'^2} + \frac{c^2}{h''^2} = 1;$$

and, because the excentricities must be equal to those of the given ellipsoid, therefore $h^2 - h'^2 = k^2 - k'^2 = e^2$, and $h''^2 - h'^2 = k''^2 - k'^2 = e''^2$: hence

$$\frac{a^2}{h^2} + \frac{b^2}{h'^2 + e^2} + \frac{c^2}{h'^2 + e''^2} = 1;$$

an equation which now contains only one unknown quantity,

namely, h. It is plain that one value of h, and only one, may, in all cases, be determined from this equation. For, by taking h small enough, the function on the left hand side will become greater than any positive quantity how great soever; and by taking h great enough, the same function will become less than any positive quantity how small soever: and while h increases from o, *ad infinitum*, the function continually decreases from being infinitely great to be infinitely little. Therefore there is only one ellipsoid, having the required conditions, whose surface will pass through the attracted point.* When h is determined, then $h' = \sqrt{h^2 + c^2}$, $h'' = \sqrt{h^2 + c^2}$: and in consequence of the equation,

$$\frac{a^2}{h^2} + \frac{b^2}{h'^2} + \frac{c^2}{h''^2} = 1,$$

we may suppose, $a = h \cos. m$, $b = h' \sin. m \cos. n$, $c = h'' \sin. m \sin. n$.

Let these values of a, b, c be substituted in the last expressions for Δ and Δ'; then

$$\Delta = \left\{ (h \cos. m - k \cos. \varphi)^2 + (h' \sin. m \cos. n - k' \sin. \varphi \cos. \psi)^2 + (h'' \sin. m \sin. n - k'' \sin. \varphi \sin. \psi)^2 \right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{ (h \cos. m + k \cos. \varphi)^2 + (h' \sin. m \cos. n - k' \sin. \varphi \cos. \psi)^2 + (h'' \sin. m \sin. n - k'' \sin. \varphi \sin. \psi)^2 \right\}^{\frac{1}{2}}:$$

and because $h'^2 = h^2 + c^2$, $h''^2 = h'^2 + c'^2$, $k'^2 = k^2 + c^2$, $k''^2 = k^2 + c'^2$, we shall readily obtain

$$\Delta = \left\{ h^2 - 2hk \cos. m \cos. \varphi - 2h'k' \sin. m \cos. n \sin. \varphi \cos. \psi - 2h''k'' \sin. m \sin. n \sin. \varphi \sin. \psi + k^2 + c^2 \sin.^2 m \cos.^2 n + c'^2 \sin.^2 m \sin.^2 n + c^2 \sin.^2 \varphi \cos.^2 \psi + c'^2 \sin.^2 \varphi \sin.^2 \psi \right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{ h^2 + 2hk \cos. m \cos. \varphi - 2h'k' \sin. m \cos. n \sin. \varphi \cos. \psi \right.$$

* *Mecan. Celeste*, Tom. II. p. 50.

$+ 2k'k''$ sin. m sin. n sin. ϕ sin. $\psi + k'^2 + e'^2$ sin.$^2 m$ cos.$^2 n + e''^2$ sin.$^2 m$ sin.$^2 n + e'^2$ sin.$^2 \phi$ cos.$^2 \psi + e''^2$ sin.$^2 \phi$ sin.$^2 \psi \Big\}^{\frac{1}{2}}$.

In these values of Δ and Δ', it is plain that the quantities h, h', h'' are alike concerned with the quantities k, k', and k'': and hence, by interchanging the semi-axes of the two ellipsoids, we may represent each of the expressions for Δ and Δ' in two forms, which, when expanded, are identical: thus

$$\Delta = \Big\{(h \cos. m - k \cos. \phi)^2 + (h' \sin. m \cos. n - k' \sin. \phi \cos. \psi)^2 + (h'' \sin. m \sin. n - k'' \sin. \phi \sin. \psi)^2\Big\}^{\frac{1}{2}} = \Big\{(k \cos. m - h \cos. \phi)^2 + (k' \sin. m \cos. n - h' \sin. \phi \cos. \psi)^2 + (k'' \sin. m \sin. n - h'' \sin. \phi \sin. \psi)^2\Big\}^{\frac{1}{2}},$$

$$\Delta' = \Big\{(h \cos. m + k \cos. \phi)^2 + (h' \sin. m \cos. n - k' \sin. \phi \cos. \psi)^2 + (k' \sin. m \sin. n - k'' \sin. \phi \sin. \psi)^2\Big\}^{\frac{1}{2}} = \Big\{(k \cos. m + h \cos. \phi)^2 + (k' \sin. m \cos. n - h' \sin. \phi \cos. \psi)^2 + (k'' \sin. m \sin. n - h'' \sin. \phi \sin. \psi)^2\Big\}^{\frac{1}{2}}.$$

In the formula (3)

$$A = Vk'' \iint \sin. \phi \cos. \phi \, . \, d\phi \, . \, d\psi \Big\{\frac{1}{\Delta} - \frac{1}{\Delta'}\Big\},$$

the symbols Δ and Δ' express the distances of the attracted point, situated in the surface of the ellipsoid whose semi-axes are h, h', h'', and determined by the co-ordinates a, b, c, or $h \cos. m$, $h' \sin. m \cos. n$, $h'' \sin. m \sin. n$, from the extremities of a prism of the matter of the ellipsoid first considered, parallel to the axis k, and having $k' k'' \sin. \phi \cos. \phi \, . \, d\phi \, . \, d\psi$ for its base, and its length equal to $2k \cos. \phi$: and, if we take a point in the surface of the last mentioned ellipsoid, that shall have $k \cos. m$, $k' \sin. m \cos. n$, $k'' \sin. m \sin. n$ (which we may denote by a', b', c') for its co-ordinates; and conceive a prism of the matter of the other ellipsoid, parallel to k and h, that

shall have $k'k''\sin.\phi\cos.\phi.d\phi.d\psi$ for its base, and its length equal to $2h\cos.\phi$; then, it is a consequence of what has been shown above, that Δ and Δ' will likewise express the distances of the point, having a', b', c' for its co-ordinates from the extremities of this last prism. Therefore, if we put

$$A' = k'k'' \iint \sin.\phi\cos.\phi.d\phi.d\psi \left\{\frac{1}{\Delta} - \frac{1}{\Delta}\right\};$$

then will A' (when the double fluent is taken between the same limits as in the case of A) be equal to the attractive force which the ellipsoid of homogeneous matter, whose semi-axes are h, h', h'', exerts on the point, whose co-ordinates are $k\cos.m$, $k'\sin.m\cos.n$, $k''\sin.m\sin.n$, or a', b', c', in the direction parallel to the axis h. For, in the formula for A, as the fluxion under the sign of double integration, denotes the attractive force of an indefinitely small prism of the matter of the ellipsoid, whose semi-axes are k, k', k'' upon the point whose co-ordinates are a, b, c, in the direction parallel to k and h; so, for the like reasons, in the formula for A', the fluxion under the same sign, will denote the attractive force of an indefinitely small prism of the matter of the ellipsoid, whose semi-axes are h, h', h'', upon the point whose co-ordinates are a', b', c': and therefore the two fluents, when extended to all the prisms that compose the ellipsoids, will denote the attractions of the whole masses upon the respective points, in the direction mentioned. Thus the attractions A and A' depend upon the same fluent, and they are manifestly in the same proportion as $k'k''$ is to $h'h''$.

And if we denote by B' and C' the attractive forces which the ellipsoid of homogeneous matter, whose semi-axes are h, k', k'' exerts on the point whose co-ordinates are a', b', c', in

the directions parallel to k' and k''; it may, in like manner, be shewn, that the attractions B and B′ have the same proportion as kk'' has to hh''; and the attractions C and C′, the same proportion as kk' to hh'.

The points in the surfaces of the two ellipsoids, which are determined by the co-ordinates, $h \cos. m$, $h' \sin. m \cos. n$, $h'' \sin. m \sin. n$, or a, b, c, and $k \cos. m$, $k' \sin. m \cos. n$, $k'' \sin. m \sin. n$, or a', b', c', may not improperly be called corresponding points of the surfaces: they are such points as are situated on the same sides of the planes of the principal sections, and have their co-ordinates respectively proportional to the axes to which they are parallel. This being premised, the result of the foregoing investigation may be enunciated, as in the following theorem:

"If two ellipsoids of the same homogeneous matter have "the same excentricities, and their principal sections in the "same planes; the attractions which one of the ellipsoids ex- "erts upon a point in the surface of the other, perpendicularly "to the planes of the principal sections, will be to the attrac- "tions which the second ellipsoid exerts upon the correspond- "ing point in the surface of the first, perpendicularly to the "same planes, in the direct proportion of the surfaces, or "areas, of the principal sections to which the attractions are "perpendicular."

For the principal sections, being ellipses, their areas are proportional to the products of the semi-axes.

When the attracted point, of which the co-ordinates are a, b, c, is placed without the ellipsoid having k, k', k'' for its semi-axes; then the point, of which a', b', c' are the co-ordinates, is necessarily within the other ellipsoid: and, on account of the

relation which has been shewn to take place between the attractions of the two solids upon corresponding points in one another's surfaces, the case, when the attracted point is placed without an ellipsoid, is made to depend upon the case, when the attracted point is within the surface.

4. Let us now consider the formula (2) for the attractive force parallel to the axis x,

$$A = \iint dy \cdot dz \left\{ \frac{1}{\Delta} - \frac{1}{\Delta'} \right\}$$

on the supposition that the attracted point is within the ellipsoid. If $a = o$ (that is, if the attracted point be in the plane of y and z) then $\frac{1}{\Delta} - \frac{1}{\Delta'} = o$, for all values of x, y, and z: and, in this case, the whole attractive force A is evanescent, as it ought to be. For all other values of a, the expression $\frac{1}{\Delta} - \frac{1}{\Delta'}$, in the circumstances supposed, is plainly a finite positive quantity: and, therefore, supposing b and c to be constant, and a to increase, we must infer that the attractive force A will receive finite increments, so long as the point determined by the co-ordinates a, b, c, is within the ellipsoid. If this point be in the surface, then the variable ordinates x, y, z, when they belong to points indefinitely near to the attracted point, will approach indefinitely to an equality with a, b, c; and the corresponding values of $\frac{1}{\Delta} - \frac{1}{\Delta'}$, and, consequently, the fluxions of the force A, will become infinitely great; on which account the continuity of the function A is broken off. From what has now been observed, it follows, that we may substitute for the force A, its expansion in a series of the powers of a, provided we are careful not to extend the conclusions obtained by reasoning from the nature of such series,

to the case when the attracted point is without the surface of the ellipsoid.

Let $R' = x' + (b-y)' + (c-z)'$, then

$$\Delta = \left\{R' + a(a-2x)\right\}^{\frac{1}{2}}$$

$$\Delta' = \left\{R' + a(a+2x)\right\}^{\frac{1}{2}};$$

and, if the function $\frac{1}{\Delta} - \frac{1}{\Delta'}$ be expanded into a series, the terminus generalis of that series will be

$$\frac{\left[\frac{1}{2}\cdot\frac{3}{4}\cdot\frac{5}{6}\ldots\right]a^n}{1\cdot 2\cdot 3\ldots n}\left(\frac{d^n}{dx^n}\right)\frac{(a+2x)^n - (a-2x)^n}{R^{n+\frac{1}{2}}};$$

and, hence it is plain, that all the even powers of a will disappear, and only the odd powers will remain. Now, the expansion of the force A cannot contain any of the powers of a, excepting those which enter into the series for $\frac{1}{\Delta} - \frac{1}{\Delta'}$: therefore, supposing the expansion of A to be arranged according to the powers of a, it will necessarily be of this form, *viz.*

$$A = A^{(1)}a + A^{(3)}a^3 + A^{(5)}a^5 + A^{(7)}a^7 + \&c.;$$

where $A^{(1)}$, $A^{(3)}$, $A^{(5)}$, &c. are functions independent of a. The first of these coefficients, it is easy to prove, will be determined by this formula,

$$A^{(1)} = fff \frac{x\,.\,dy\,.\,dz}{\left\{x' + (b-y)' + (c-z)'\right\}^{\frac{3}{2}}}; \quad (4)$$

and, with regard to the rest, they may be all shewn to depend on $A^{(1)}$, in consequence of an equation in partial fluxions, first noticed by La Place, and derived from the nature of the functions under consideration. In effect, the truth of the following formulas will be established by merely performing the operations indicated, *viz.*

$$\left(\frac{d^2.\frac{1}{\Delta}}{dx^2}\right) + \left(\frac{d^2.\frac{1}{\Delta}}{dy^2}\right) + \left(\frac{d^2.\frac{1}{\Delta}}{dz^2}\right) = 0$$

$$\left(\frac{da.\frac{1}{\Delta}}{da^2}\right)+\left(\frac{da.\frac{1}{\Delta}}{db^2}\right)+\left(\frac{da.\frac{1}{\Delta}}{dc^2}\right)=0$$

and hence it is easy to infer, that

$$\left(\frac{d^2A}{da^2}\right)+\left(\frac{d^2A}{db^2}\right)+\left(\frac{d^2A}{dc^2}\right)=0.$$

Substitute the series for A in this last equation, and let the coefficients of the several powers of a be equated to 0; and there will be obtained

$$A^{(3)}=-\frac{1}{2.3}\times\left\{\left(\frac{d^2A^{(1)}}{db^2}\right)+\left(\frac{d^2A^{(1)}}{dc^2}\right)\right\}$$

$$A^{(5)}=-\frac{1}{4.5}\left\{\left(\frac{d^2A^{(3)}}{db^2}\right)+\left(\frac{d^2A^{(3)}}{dc^2}\right)\right\}$$

$$A^{(7)}=-\frac{1}{6.7}.\left\{\left(\frac{d^2A^{(5)}}{db^2}\right)+\left(\frac{d^2A^{(5)}}{dc^2}\right)\right\},$$

&c.

Thus, all the other coefficients depend upon the coefficient of the first term, being derived from it by a repetition of the same operations: and when the general expression of $A^{(i)}$ shall be determined, the whole series will become known.

Resume the formula (4)

$$A^{(1)}=\iint\frac{dx.dy.dz}{\left\{x^2+(b-y)^2+(c-z)^2\right\}^{\frac{1}{2}}}.A$$

and let

$$x=R\cos.p$$

$$b-y=R\sin.p\cos.q$$

$$c-z=R\sin.p\sin.q.$$

then will $R=\left\{x^2+(b-y)^2+(c-z)^2\right\}^{\frac{1}{2}}$ express the line drawn from the foot of a to the point in the surface of the ellipsoid, of which x, y, z are the co-ordinates; p will be the angle which R makes with a; and q the angle which the plane drawn through R and a, makes with the plane of y and x. In

consequence of the equation of the solid, R is a function of the angles p and q: therefore, making p only variable, we shall have

$$-dy = \left\{\left(\frac{dR}{dp}\right)\sin. p + R\cos. p\right\}\cos. q\,.\,dp:$$

then, because y must be constant when x varies, we must make

$$-dx = \left\{\left(\frac{dR}{dp}\right)\sin. p + R\cos. p\right\}\sin. q\,.\,dp + \left\{\left(\frac{dR}{dq}\right)\sin. q + R\cos. q\right\}\sin. p\,.\,dq,$$

$$o = \left\{\left(\frac{dR}{dp}\right)\sin. p + R\cos. p\right\}\cos. q\,.\,dp + \left\{\left(\frac{dR}{dq}\right)\cos. q - R\sin. q\right\}\sin. p\,.\,dq.$$

and by extirpating dp, we get

$$-dx = \frac{R\sin. p\,.\,dq}{\cos. q}.$$

and hence, by substitution,

$$A^{(1)} = 8\iint\left\{\frac{\left(\frac{dR}{dp}\right)}{R}\cos. p\sin. p + \cos. p\sin. p\left\{\frac{dR}{dq}\,.\,dq\right.\right.;$$

the fluent to be taken from $p = o$, to $p = \frac{\pi}{2}$, and from $q = o$, $q = \frac{\pi}{2}$.

The transformed formula for $A^{(1)}$ cannot be integrated, unless we substitute, in place of R, the function of the angles p and q, that is equal to it. Now, $x = R\cos. p$, $y = b - R\sin. p\cos. q$, $z = c - R\sin. p\sin. q$: let these values be substituted in the equation of the solid,

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1;$$

and, for the sake of simplicity, let

$$M = \frac{\cos.^2 p}{a^2} + \frac{\sin.^2 p \cos.^2 q}{b^2} + \frac{\sin.^2 p \sin.^2 q}{c^2},$$

$$N = \frac{b \sin.^2 p \cos. q}{b^2} + \frac{c \sin.^2 p \sin. q}{c^2},$$

$$D = 1 - \frac{b^2}{a^2} - \frac{c^2}{a^2} + \sin.^2 p \left\{ \left(\frac{b}{a}\right) \right\} =$$

then

$$R^2 - 2 \cdot \frac{N}{M} \cdot R - \frac{D}{M} = 0.$$

This equation has two roots, *viz.*

$$R = \frac{\pm \sqrt{N^2 + MD} + N}{M};$$

and, because D is always positive when the attracted point is within the solid, as is here supposed, both these roots are real quantities, whatever be the angles p and q. Conceive the line R to be produced to meet the surface of the ellipsoid again below the plane of y and z, then, if the produced part be denoted by R′, it is plain that R and R′ will be the two roots of the above equation: and because R′, although in an opposite direction, has the same angular position as R, we may substitute R′ for R, in the expression for $A^{(1)}$: thus,

$$A^{(1)} = 2 \iint \left\{ \frac{\left(\frac{dR'}{dp}\right)}{R'} \cos. p \sin.^2 p + \cos.^2 p \sin. p \right\} dp \cdot dq.$$

Therefore, by adding together the two values of $A^{(1)}$, and taking half the sum, we get

$$A^{(1)} = \iint \left\{ \left(\frac{\left(\frac{dR}{dp}\right)}{R} + \frac{\left(\frac{dR'}{dp}\right)}{R'} \right) \cos. p \sin.^2 p + 2 \cos.^2 p \sin. p \right\} dp \, dq,$$

or

$$A^{(1)} = \iint \left\{ \frac{\left(\frac{d \cdot RR'}{dp}\right)}{RR'} \cos. p \sin.^2 p + 2 \cos.^2 p \sin. p \right\} dp \cdot dq :$$

the limits of this fluent being, as before, from $p = 0$ to $p = \frac{\pi}{2}$, and from $q = 0$ to $q = 2\pi$.

By the theory of equations $RR' = -\frac{D}{M}$: and, by substitution, the last expression of $A^{(1)}$ will become

$$A^{(1)} = \iint \left\{ -\frac{\left(\frac{dM}{dz}\right)}{M} \cos.p \sin.^2 p + 2 \cos.^2 p \sin.p \right\} dp \,.\, dq.$$

It is remarkable, that the last expression of $A^{(1)}$ does not contain either of the quantities b or c; for these do not enter into the function M: and hence we are to conclude that the value of $A^{(1)}$ is independent on these co-ordinates, and is the same for all points situated within the same principal section of the ellipsoid. Another inference is, that all the other coefficients $A^{(3)}$, $A^{(5)}$, &c. of the expansion of the force A are severally equal to 0, as is plain from the law which connects those quantities with one another, and with $A^{(1)}$: on this account the expansion alluded to will be reduced to its first term, and we shall have, simply,

$$A = A^{(1)} \times a.$$

The same considerations likewise suggest a new analytical expression of $A^{(1)}$; which, on account of its simplicity, and its immediate dependence on the figure and equation of the solid, seems to deserve the preference to every other: for, since it has been shewn that the value of $A^{(1)}$ is independent on the co-ordinates b and c, we may exterminate these quantities from the formula (4); and thus

$$A^{(1)} = \iint \frac{xz \,.\, dy \,.\, dz}{\left\{x^2 + y^2 + z^2\right\}^{\frac{3}{2}}};$$

the fluent to be extended to the whole of the surface of the principal section made by the plane of y and z.

The same reasoning that has been applied to the determi-

nation of the attractive force A, it is evident, will apply equally to the attractions denoted by B and C: and, therefore, the attractions of an ellipsoid, acting perpendicularly to the planes of the principal sections, upon a point situated within the surface, are as follows, *viz.*

$$A = a \times \iiint \frac{x^2\,dy\,dz}{\left\{x^2 + y^2 + z^2\right\}^{\frac{1}{2}}}$$

$$B = b \times \iiint \frac{y^2\,dx\,dz}{\left\{x^2 + y^2 + z^2\right\}^{\frac{1}{2}}} \qquad (5)$$

$$C = c \times \iiint \frac{z^2\,dx\,dy}{\left\{x^2 + y^2 + z^2\right\}^{\frac{1}{2}}}.$$

the several fluents to be extended to the whole of the surfaces of the principal sections, to which the attractions are perpendicular.

When the attracted point is without the ellipsoid, it becomes necessary, in the first place, to determine the semi-axes of another ellipsoid whose surface shall pass through the attracted point, and which shall have the same excentricities and its principal sections in the same planes, as the given ellipsoid: these semi-axes have been denoted by h, h', h'', and the formulas for computing them have already been given.* We must next determine the co-ordinates of the point in the surface of the given ellipsoid, that corresponds to the attracted point in the surface of the other ellipsoid: and, according to the definition that has been given of them, these co-ordinates, denoted by a', b', c' are thus found; $a' = a \times \frac{h}{k}$; $b' = b \times \frac{h'}{k'}$; $c' = c \times \frac{h''}{k''}$.† These things being determined, the attractions of the ellipsoid whose semi-axes are h, h', h'', upon the point whose

* Pages 351 and 352. † Page 355.

co-ordinates are a', b', c' (which is plainly within the solid) are as follows:

$$A' = a \times \frac{A}{L} \times \iiint \frac{dx' \cdot dy' \cdot dz'}{\left\{x'^2 + y'^2 + z'^2\right\}^{\frac{1}{2}}}$$

$$B' = b \times \frac{B}{M} \times \iiint \frac{dx' \cdot dy' \cdot dz'}{\left\{x'^2 + y'^2 + z'^2\right\}^{\frac{1}{2}}}$$

$$C' = c \times \frac{C}{N} \times \iiint \frac{dx' \cdot dy' \cdot dz'}{\left\{x'^2 + y'^2 + z'^2\right\}^{\frac{1}{2}}};$$

where x', y', z' are the three co-ordinates of a point in the surface of the ellipsoid, whose semi-axes are A, B', C'. To determine the attractions of the given ellipsoid upon the given point, we have now only to apply the theorem demonstrated in § 3; and so,

$$A = a \times \frac{ABC}{LMN} \cdot \iiint \frac{dx' \cdot dy' \cdot dz'}{\left\{x'^2 + y'^2 + z'^2\right\}^{\frac{1}{2}}}$$

$$B = b \times \frac{ABC}{LMN} \cdot \iiint \frac{dx' \cdot dy' \cdot dz'}{\left\{x'^2 + y'^2 + z'^2\right\}^{\frac{1}{2}}} \qquad (5)$$

$$C = c \times \frac{ABC}{LMN} \cdot \iiint \frac{dx' \cdot dy' \cdot dz'}{\left\{x'^2 + y'^2 + z'^2\right\}^{\frac{1}{2}}}.$$

5. If we examine the expressions (5) for the attractions of an ellipsoid upon a point placed within the surface, it will readily appear that the coefficients, into which the co-ordinates of the attracted point are multiplied, are homogeneous functions of 0 dimensions of the semi-axes of the solid, these quantities rising to the same dimensions in the numerators of the functions, as in the denominators: and hence it is easy to infer, that the values of these coefficients depend only on the proportions of the semi-axes to one another, and not at all upon their absolute magnitudes. Therefore, if we conceive two ellipsoids of the same homogeneous matter, similar to

one another and similarly placed, whose surfaces envelop the same attracted point; it is plain, from what has just been remarked, that the attractions of these ellipsoids upon the point will be precisely equal. Thus it appears, that the matter inclosed between the surfaces of the two solids, does not alter the attractive force of the inner ellipsoid; which could not be the case, unless the attraction of the superadded matter in any one direction were precisely equal to the attraction of the same matter in the contrary direction, so as to produce an equilibrium of opposing forces. Hence we may extend to a shell of homogeneous matter, bounded by any finite surfaces of the second order, which are similar to one another and similarly placed, what Sir ISAAC NEWTON has demonstrated in the like hypothesis for surfaces of revolution;* as in the following theorem:

"If a point be situated within a shell of homogeneous mat-
"ter, bounded by two finite surfaces of the second order,
"which are similar and similarly placed; then the attraction
"of the matter of the shell upon the point, in any one direc-
"tion, will be equal to, and destroy, the attraction of the same
"matter, in the opposite direction."

6. Nothing more is wanting to complete a theory of the attractions of homogeneous ellipsoids, than to integrate the fluxional expressions (5) already obtained. In the case of a sphere, we have $k = k' = k''$, and $x^2 + y^2 + z^2 = k^2$; therefore

$$A = a \times \iint \frac{2x \,.\, dy \,.\, dz}{k^2};$$

now $2x \,.\, dy \,.\, dz$ is equal to a prism of the matter of the solid,

* Prin. Math. Lib. I. Prop. 70, Prop. 91. Cor. 3.

whose length is sx and its base $dy \cdot dz$; and hence $\iint sx \cdot dy \cdot dz$, taken within the limits prescribed, is no other than the mass of the sphere $= \frac{4\pi}{3} \cdot h^3$. Therefore $A = \frac{4\pi}{3} \times a$.

The same reasoning, it is evident, will apply to the remaining attractions B and C: and hence the attractions of a sphere upon a point within the surface, acting perpendicularly to the planes of any three great circles that intersect at right angles, are thus expressed,

$$A = a \times \frac{4\pi}{3}$$

$$B = b \times \frac{4\pi}{3}$$

$$C = c \times \frac{4\pi}{3}$$

These three forces compose a force, directed to the centre of the sphere, and equal to $\frac{4\pi}{3} \times \sqrt{a^2 + b^2 + c^2}$: it is therefore directly proportional to the distance from the center.

For a point without the surface of a sphere, we have $h = h' = h'' = \sqrt{a^2 + b^2 + c^2}$: hence it is easy to infer, that the formulas (6) will become,

$$A = a \times \frac{\frac{4\pi}{3} \cdot h^3}{(a^2 + b^2 + c^2)^{\frac{3}{2}}} = \frac{a \times M}{(a^2 + b^2 + c^2)^{\frac{3}{2}}}$$

$$B = b \times \frac{\frac{4\pi}{3} \cdot h^3}{(a^2 + b^2 + c^2)^{\frac{3}{2}}} = \frac{b \times M}{(a^2 + b^2 + c^2)^{\frac{3}{2}}}$$

$$C = c \times \frac{\frac{4\pi}{3} \cdot h^3}{(a^2 + b^2 + c^2)^{\frac{3}{2}}} = \frac{c \times M}{(a^2 + b^2 + c^2)^{\frac{3}{2}}}$$

where $M = \frac{4\pi}{3} \cdot h^3 =$ the mass of the sphere. These three forces compose a force $= \frac{M}{a^2 + b^2 + c^2}$, directed to the center:

this force is, therefore, directly as the mass, and inversely as the square of the distance from the center of the sphere.

For an ellipsoid in general, we have $x = k \cos. \varphi$, $y = k' \sin. \varphi \cos. \psi$, $z = k'' \sin. \varphi \sin. \psi$: in order to transform the formulas (5), we must first compute the values of $dy \cdot dz$, $dx \cdot dz$, $dx \cdot dy$. For this purpose, let the fluxion of y be taken, making φ the only variable, so that $dy = k' \cos. \varphi \cos. \psi \times d\varphi$: then, because y must be constant in the expression of the force A, when x varies, we must make

$$dx = k'' \cos. \varphi \sin. \psi \cdot d\varphi + k'' \sin. \varphi \cos. \psi \cdot d\psi$$
$$o = k' \cos. \varphi \cos. \psi \cdot d\varphi - k' \sin. \varphi \sin. \psi \cdot d\psi,$$

and, by exterminating $d\varphi$, we get $dx = k'' \frac{\sin. \varphi}{\cos. \psi} \times d\psi$: therefore

$$dy \cdot dz = kk'' \cos. \varphi \sin. \varphi \cdot d\varphi \cdot d\psi.$$

Again, because the value of x depends only on the angle φ, we have $dx = -k \sin. \varphi \cdot d\varphi$: and, by taking the fluxions of y and z relatively to the variable ψ, we have $dy = -k' \sin. \varphi \sin. \psi \cdot d\psi$, $dz = k'' \sin. \varphi \cos. \psi \cdot d\psi$: therefore,

$$dx \cdot dy = kk' \sin.^2 \varphi \sin. \psi \cdot d\varphi \cdot d\psi$$
$$dx \cdot dz = kk'' \sin.^2 \varphi \cos. \psi \cdot d\varphi \cdot d\psi:$$

in these expressions the sign $-$, which stands before the values of dx and dy, has been neglected: for that sign marks only that x and y decrease when the angles φ and ψ increase, and does not affect the absolute magnitudes of the fluents, which are alone the subjects of our research. Observing that $k'^2 = k^2 + c^2$, and $k''^2 = k^2 + c'^2$, the formulas (5) will now become, by substitution,

$$A = a \times 2kk'k'' \times \iint \frac{\cos.^2\varphi \sin.\varphi \cdot d\varphi \cdot d\psi}{\left\{k^2 + c^2 \sin.^2\varphi \cos.^2\psi + c^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{1}{2}}}$$

$$B = b \times 2kk'k'' \times \iint \frac{\sin.^2\varphi \cos.^2\psi \cdot d\varphi \cdot d\psi}{\left\{k^2 + c^2 \sin.^2\varphi \cos.^2\psi + c^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{1}{2}}}$$

$$C = c \times 2kk'k'' \times \iint \frac{\sin.^2\varphi \sin.^2\psi \cdot d\varphi \cdot d\psi}{\left\{k^2 + c^2 \sin.^2\varphi \cos.^2\psi + c^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{1}{2}}}$$

the several fluents to be taken from $\varphi = o$ to $\varphi = \frac{\pi}{2}$, and from $\psi = o$ to $\psi = 2\pi$.

Let

$$Q = \iint \frac{\sin.^2\varphi \cdot d\varphi \cdot d\psi}{\left\{k^2 + c^2 \sin.^2\varphi \cos.^2\psi + c^2 \sin.^2\varphi \sin.^2\psi\right\}^{\frac{1}{2}}};$$

then the last values of A, B, and C will be expressed by the partial fluxions of Q, as follows:

$$A = a \times 2kk'k'' \times \left\{-\frac{1}{k}\left(\frac{dQ}{dk}\right) + \frac{1}{c^2}\left(\frac{dQ}{dc}\right) + \frac{1}{c^2}\left(\frac{dQ}{d\psi}\right)\right\}$$

$$B = b \times 2kk'k'' \times -\frac{1}{c^2}\left(\frac{dQ}{d\psi}\right)$$

$$C = c \times 2kk'k'' \times -\frac{1}{c^2}\left(\frac{dQ}{d\psi}\right).$$

For the sake of brevity, let $k' = c' \cos.^2\psi + c^2 \sin.^2\psi$: then

$$-\left(\frac{dQ}{dk}\right) = \iint \frac{2 \sin.\varphi \cdot d\varphi \cdot d\psi}{(k^2 + k' \sin.^2\varphi)^{\frac{3}{2}}};$$

and, by integrating relatively to φ,

$$-\left(\frac{dQ}{dk}\right) = \int \frac{d\psi}{k^2 + c^2} \cdot \left\{1 - \frac{k \cos.\varphi}{(k^2 + k' \sin.^2\varphi)^{\frac{1}{2}}}\right\};$$

and, by taking the whole fluent from $\varphi = o$ to $\varphi = \frac{\pi}{2}$, and restoring the value of k',

$$-\left(\frac{dQ}{dk}\right) = \int \frac{d\psi}{k^2 + c^2 \cos.^2\psi + c^2 \sin.^2\psi}.$$

Let $\tau = \left(\frac{k^2 + c^2}{k^2 + c^2}\right)^{\frac{1}{2}} \times \frac{\sin.\psi}{\cos.\psi}$: then, by substitution,

$$-\left(\frac{dQ}{dk}\right) = \frac{1}{(k^2 + c^2)^{\frac{1}{2}}(k^2 + c^2)} \int \frac{d\tau}{1 + \tau^2};$$

and, by integrating from $\psi = o$ to $\psi = 2\pi$,

hence

$$Q = 2\pi \times \int \frac{-dk}{(k^2+c^2)^{\frac{1}{2}}(k^2+c'^2)^{\frac{1}{2}}(k^2+c''^2)^{\frac{1}{2}}};$$

the fluent to be taken so as to vanish when k is infinitely great; because Q decreases, when k increases, and the former quantity is infinitely small, when the latter is infinitely great. From this value of Q, we get

$$-\frac{1}{a}\left(\frac{dQ}{da}\right) = 2\pi \times \int \frac{-dk}{(k^2+a^2)^{\frac{1}{2}}(k^2+c'^2)^{\frac{1}{2}}(k^2+c''^2)^{\frac{1}{2}}}$$

$$-\frac{1}{b}\left(\frac{dQ}{db}\right) = 2\pi \times \int \frac{-dk}{(k^2+b^2)^{\frac{1}{2}}(k^2+c'^2)^{\frac{1}{2}}(k^2+c''^2)^{\frac{1}{2}}}$$

$$-\frac{1}{c}\left(\frac{dQ}{dc}\right) = \frac{2\pi}{1} \cdot \frac{1}{(k^2+c^2)^{\frac{1}{2}}(k^2+c'^2)^{\frac{1}{2}}(k^2+c''^2)^{\frac{1}{2}}};$$

and from these it is easy to infer that

$$-\frac{1}{a}\left(\frac{dQ}{da}\right)+\frac{1}{b}\left(\frac{dQ}{db}\right)+\frac{1}{c}\left(\frac{dQ}{dc}\right) = 2\pi \times \int \frac{-dk}{(k^2+a^2)^{\frac{1}{2}}(k^2+b^2)^{\frac{1}{2}}(k^2+c^2)^{\frac{1}{2}}};$$

therefore, if $M = \frac{4\pi}{3} \cdot kk'k'' =$ the mass of the ellipsoid, the last formulas for A, B, C will become, by substitution,

$$A = 3aM \times \int \frac{-dk}{(k^2+a^2)^{\frac{1}{2}}(k^2+b^2)^{\frac{1}{2}}(k^2+c^2)^{\frac{1}{2}}}$$

$$B = 3bM \times \int \frac{-dk}{(k^2+b^2)^{\frac{1}{2}}(k^2+c^2)^{\frac{1}{2}}(k^2+a^2)^{\frac{1}{2}}} \quad (7)$$

$$C = 3cM \times \int \frac{-dk}{(k^2+c^2)^{\frac{1}{2}}(k^2+a^2)^{\frac{1}{2}}(k^2+b^2)^{\frac{1}{2}}}$$

all these different fluents are to be conceived, as beginning to increase when k is infinitely great, and are to be extended till k has decreased, so as to be equal to the least of the semi-axes of the ellipsoid. In the general case of the problem, the expressions that have been obtained transcend the limits of the ordinary analysis; and their integration requires the introduction of other quantities besides algebraic expressions and circular arcs and logarithms. They belong to the class of elliptical transcendants; a branch of the mathematics which has been

very successfully cultivated, and is fertile in resources and methods that are applicable to every particular instance.

The fluents in the formulas (6.) for a point without the surface, are derived from the ellipsoid whose semi-axes are h, h', h'', in the same manner as the fluents already considered are derived from the given ellipsoid; and, because $\frac{kk'k''}{hh'h''}$ is equal to the mass of the latter solid, divided by the mass of the former one, it is easy to infer that we have only to substitute h for k in the fluents of the formulas (7), to obtain the expressions of the attractions of the given ellipsoid upon a point without the surface. Thus the two cases, when the attracted point is within the solid or in the surface, and when it is without the solid, differ only in the limits of the fluents: in the former case, the fluents, beginning when the variable quantity is infinitely great, are to be extended till it has decreased, so as to be equal to the least of the semi-axes of the given ellipsoid; and, in the latter case, the fluents are to be extended only till the variable quantity has decreased, so as to be equal to h, the least of the semi-axes of the ellipsoid, whose surface passes through the attracted point. In the former case, the values of the fluents are the same for all points within the ellipsoid, and in its surface; in the latter case, these values depend upon the position of the attracted point.

The preceding formulas, being founded on the most general hypothesis, are applicable to all figures bounded by finite surfaces of the second order. The case of the sphere, which corresponds to the supposition that the excentricities ϵ' and ϵ'' are both evanescent, has already been considered, and, as it is attended with no difficulty, it needs not be again discussed:

but the two cases of solids of revolution, that of the oblate and oblong spheroids, are deserving of particular attention.

In the oblate spheroid, the two greater semi-axes b' and b'' are equal to one another; and, therefore, it corresponds to the supposition of $c' = c''$. In this case the formulas (7) will become

$$A = 3qM \cdot \int \frac{-\,dt}{b^2(b^2+t)^2}$$

$$B = 3bM \cdot \int \frac{-\,dt}{(b^2+t)^2}$$

$$C = 3cM \cdot \int \frac{-\,dt}{(b^2+t)^2};$$

these expressions may be all integrated by the ordinary methods, and thus we get

$$A = \frac{3qM}{b^2} \cdot \left\{ \frac{c}{k} - \text{arc. tan.} \frac{c}{k} \right\}$$

$$B = \frac{3bM}{4k^2} \cdot \left\{ \text{arc. tan.} \frac{c}{k} - \frac{\frac{c}{k}}{1+\frac{c^2}{k^2}} \right\}$$

$$C = \frac{3cM}{4k^2} \cdot \left\{ \text{arc. tan.} \frac{c}{k} - \frac{\frac{c}{k}}{1+\frac{c^2}{k^2}} \right\}.$$

The formulas express the attractions of an oblate spheroid upon a point within the surface or in it, acting parallel to a, b, c, the co-ordinates of that point, of which a is parallel to the axis of revolution.

When the attracted point is without the surface, we have only to compute k, the semi-axis of revolution of the spheroid, whose surface passes through the attracted point, and to substitute it for k in the last formulas, in order to have the expressions of the attractions sought: and it is to be remarked, that the equation for finding k, which in the general case of the

ellipsoid rises to the third degree, is, in this case, depressed to a quadratic. In effect, the equation for h, when $a' = a''$, becomes

$$\frac{a^2}{h^2} + \frac{b^2 + c^2}{b^2 + c^2 - h^2} = 1,$$

whence

$$h'' = (a^2 + b^2 + c^2 - e^2)h^2 = a^2 e^2,$$

and so

$$2h^2 = a^2 + b^2 + c^2 - e^2 + \sqrt{(a^2 + b^2 + c^2 - e^2)^2 + 4a^2 e^2}.$$

In the oblong spheroid, one of the semi-axes h' and h'' must be made equal to the least semi-axis k, which corresponds to the suppposition of $e'' = 0$. In this case, the formulas (7) will become

$$A = 3a^2 M \cdot \int \frac{-dk}{k^2(k^2 + e^2)^{\frac{1}{2}}}$$

$$B = 3b^2 M \cdot \int \frac{-dk}{k(k^2 + e^2)^{\frac{1}{2}}}$$

$$C = 3c^2 M \cdot \int \frac{-dk}{k^2(k^2 + e^2)^{\frac{1}{2}}}$$

In these expressions k is the radius of the equatorial circle of the spheroid, and not the semi-axis of revolution, which is $= \sqrt{k^2 + e^2}$; and if we change k to denote the semi-axis of revolution, which requires that $\sqrt{k^2 - e^2}$ be substituted for k; and, for the sake of uniformity with the formulas for the oblate spheroid, likewise interchange a and b, and A and B, in order that a may denote the ordinate parallel to the axis of revolution, and that A may express the attractive force in the same direction; then, the last expressions will become

$$A = 3a^2 M \cdot \int \frac{-dk}{k^2(k^2 - e^2)^{\frac{1}{2}}}$$

$$B = 3b^2 M \cdot \int \frac{-dk}{(k^2 - e^2)^{\frac{1}{2}}}$$

$$C = 3c^2 M \cdot \int \frac{-dk}{(k^2 - e^2)^{\frac{1}{2}}};$$

* Page 351.

which differ from the formulas for the oblate spheroid only in the sign of e^2, as, it is manifest, ought to be the case. By integrating, we get

$$A = \frac{3aM}{e^3} \cdot \left\{ \frac{1}{2} \cdot \text{hyp. log.} \left(\frac{1+e}{1-e} \right) - \frac{e}{1} \right\}$$

$$B = \frac{3bM}{2e^3} \cdot \left\{ \frac{\frac{e}{1}}{1-\frac{e^2}{1}} - \frac{1}{2} \cdot \text{hyp. log.} \left(\frac{1+e}{1-e} \right) \right\}$$

$$C = \frac{3cM}{2e^3} \cdot \left\{ \frac{\frac{e}{1}}{1-\frac{e^2}{1}} - \frac{1}{2} \cdot \text{hyp. log.} \left(\frac{1+e}{1-e} \right) \right\}$$

These formulas express the attractions of an oblong spheroid upon a point within the surface or in it; acting parallel to a, b, c, the co-ordinates of that point, of which a is parallel to the axis of revolution.

When the attracted point is without the spheroid, we must first compute h, the semi-axis of revolution of the spheroid, whose surface passes through the attracted point; and for this purpose we have the following expression, *viz.*

$$2h^2 = a^2 + b^2 + c^2 + e^2 + \sqrt{(a^2 + b^2 + c^2 + e^2)^2 - 4a^2e^2};$$

observing that a is the ordinate parallel to h: then the attractions required will be found merely by substituting h for b in the formulas for the case when the attracted point is within the spheroid.

XXI. *Observations on Albumen, and some other Animal Fluids; with Remarks on their Analysis by electro-chemical Decomposition.* By Mr. William Brande, *F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.*

Read June 15, 1809.

SECTION I.

Observations on Mucus and on the Composition of liquid Albumen.

The results obtained from the chemical analysis of the intervertebral fluid of the squalus maximus, an account of which is annexed to Mr. Home's paper "On the Nature of the intervertebral Substance in Fish and Quadrupeds,"* led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state, and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

1. Saliva was the first source of mucus to which I directed my attention.

In order to separate the albumen, which Dr. Bostock's analysis has shewn it to contain,† it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had

* Philosophical Transactions, 1809.

† Nicholson's Journal, Vol. XIV. page 149.

passed the filter, as a solution of nearly pure mucus; but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

One thousand grains of saliva, afforded by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

3. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol, and acids.

Finding, therefore, that the re-agents employed to detect mucus,* act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

For this purpose, I procured three glass cups, each capable of holding rather more than a measured half pince of water; one of these was filled with a mixture of equal parts of saliva

* Nitrate of silver and acetate of lead. Vide Thomson's System of Chemistry, Vol. V. page 502. 3d edition; and Nicholson's Journal, XI.—251.

and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a VOLTAIC battery of one hundred and twenty four inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water.* By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state), and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance, surrounded, and adhered to, the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

I could not at first account for this appearance, conceiving that if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

To ascertain this point, an experiment was made on the albumen of an egg.

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while

* It was conceived, that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which re-acting on the albumen would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

This result I mentioned to Mr. DAVY, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments which were undertaken to establish so probable an opinion.*

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscidity to that fluid, shewing that something is retained in solution.

Two hundred grains of the coagulated albumen of an egg, were repeatedly washed and triturated, in four ounces of distilled water, which was afterwards separated by a filter, and evaporated to about one fourth of its original bulk. It was then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue colour to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a dilute acid.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the

* On referring afterwards to Dr. THOMSON's System of Chemistry (Vol. V. page 491), I find that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.

application of electricity, from sixty four inch double plates, a copious coagulation took place at the negative pole.

So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes coagulated simply by heat.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscid fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids.* This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

It appears, therefore, that the white of egg, in its fluid state, is a compound of albumen, with alkali and water; that when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chemical combination with the albumen, is transferred to the water, and that this separation causes the coagulation of the albumen: the aqueous alkaline solution which is thus formed, re-acts upon the coagulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscid fluid already noticed.

The coagulation of albumen by alcohol and by acids, may be explained by a reference to the principles already laid down.

1. Five hundred grains of the white of egg were agitated with two ounces of pure alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquid was separated from the

* Nicholson's Journal, Vol. XI—246.

coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

In this instance then, the albumen in passing from the liquid to the solid state, gives its alkali to the alcohol.*

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and more perfectly solid, on account of their superior affinity for the alkali.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of 212°. The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another similar vessel containing a little distilled water, negatively electrified by one hundred four inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

* When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

When the electrization had been carried on in this way for one hour, the cups were removed, and their contents examined.

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5.5 grains, which had the properties of soda, in a state approaching to purity.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic crystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

This experiment shews that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda.* We learn, from the experiments of Mr. HATCHETT, that minute quantities of other saline bodies are likewise present.†

In the foregoing experiments, I had generally employed from sixty to three hundred four inch double plates of copper and zinc, but in subsequent researches, made with a view of

* May not a submuriate of soda exist in fluid albumen?

† After the destructive distillation of coagulated, dry, semi-transparent albumen, there remained "a spongy coal of very difficult incineration; as towards the end of "the process, it appeared vitrified and glazed with a melted saline coat, which was, "however, easily dissolved by water. The residuum was again exposed to a long "continued red heat, and again treated with water, till, at length, a few scarcely "visible particles remained, which as far as such a small quantity would permit to "be ascertained, proved to be phosphate of lime. The portion dissolved by water

ascertaining the action of lower powers, the effects of which I shall afterwards relate, I find that a battery of twenty-four three inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

SECTION II.

Observations on the Composition of some animal Fluids containing Albumen.

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali, is produced at the negative pole. A large portion of albumen may therefore exist in a fluid,

" (which was by much the most considerable), consisted principally of carbonate, " mixed with a small quantity of phosphate of soda.

" Five hundred grains of dry albumen afforded 74.50 grains of coal, of which 11.25 " were saline matter."

Vide " Chemical Experiments on Zeophites, with some Observations on the Com- " ponent Parts of Membrane." Phil. Trans. 1800.

incapable of separation by heat, and in the present instance, not to be detected even by acids, these re-agents producing no effect on the filtered solution, just alluded to.

2. *Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water, exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the VOLTAIC battery, a considerable and rapid coagulation takes place at the negatively electrified wire.

3. *Mucus of the Trachea, &c.*

The other varieties of mucus, as from the trachea, the nose, &c. agree with the former, in affording abundance of albumen by electric decomposition, whereas scarcely any traces of that substance can be detected by the tests of acids, heat, or alcohol.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, shewed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid, in the cases of saliva, and mucus of the trachea and nose: the mucus of the oyster afforded only soda and muriatic acid.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids, no traces of uncombined alkali (as in the white of egg) are to be detected.

These results lead to new ideas respecting the composition

of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

4. Bile.

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged throughout of a green colour, arising from the colouring matter at the same time separated.

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid, cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0.5 to three per cent., and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

The electro-chemical decomposition of this fluid, affords, besides the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids.

5. Milk.

In this fluid, the separation of albuminous matter at the negative pole, is equally evident, though not so rapid, as in most other cases. The conductors from sixty four inch double plates, highly charged, and immersed within four inches of each other in three ounces of cows milk, during one hour, produced the appearance of curds and whey, the principal part

of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup, were soda, and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids.

After such decomposition of milk, the serum still affords sugar of milk.

6. *The Liquor of the Amnios.*

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis.

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphuretted hydrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause.

Alkalies produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen.*

* The difference in the results of the analysis given in the text, and that of Vauquelin and Buniva, most probably arises from the liquor of the amnios examined by those chemists, not having been perfectly recent, and perhaps mixed with other secretions. Vide *Annales de Chimie*, XXXIII. p. 270.

7. Pus.

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by VOLTAIC electricity, takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty four four inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen, (consisting of one part of albumen to six of water), the coagulation was considerably more abundant at the negative than at the positive pole; but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

XXII. *Hints on the Subject of animal Secretions.* By Everard Home, *Esq. F. R. S. Communicated by the Society for the Improvement of Animal Chemistry.*

Read June 22, 1809.

THE brilliant discoveries of Mr. DAVY on the powers of electricity in producing chemical changes, suggested to me the

Dr. WOLLASTON's observations inserted in the Philosophical Magazine, were published after this paper had been laid before the Society.

I was led to the present investigation, while preparing my lectures on the Hunterian Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. WILLIAM BRANDE to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir JOSEPH BANKS, and stated that I should bring them forward in my lectures; at that time Dr. YOUNG's Syllabus was not published, and Dr. WOLLASTON's opinions were unknown to me.

Dr. BERZELIUS, Professor of Chemistry at Stockholm, published a work on Animal Chemistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of his opinion, the following experiment is adduced:

"Trace all the nerves leading to any secretory organ in a living animal, and divide "them, being careful to injure the blood-vessels and the structure of the organ "itself, as little as may be: notwithstanding the continued circulation of the blood, "the organ will as little secrete its usual fluid, as an eye deprived of its nerve can "see, or a muscle whose nerve has been divided can move. We may therefore easily "conceive, that any trifling alteration in the nerves of a gland, may materially affect "its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting, when we discover the insufficiency of all other explanation. Dr. BERZELIUS's work was shown to me by Mr. DAVY while this paper was in the press.

idea that the animal secretions may be produced by the same means.

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chemistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this Society, the object of which is the improvement of Animal Chemistry, their intimate acquaintance with these branches of science, renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. DAVY's discoveries can be applied, and he has given it the consideration it deserves.

The VOLTAIC battery is met with in the torpedo and electrical eel, and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts, one, that a VOLTAIC battery can be formed in a living animal, the other, that nerves are essentially necessary for its management; for in these fish, the nerves connected with the electrical organs, exceed those that go to all the other parts of the fish, in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a VOLTAIC battery of high power; but their structure appears to Mr. DAVY to adapt them to receive and preserve a small electrical power.

That the nerves arranged with muscles, so as to form a VOLTAIC battery, have a power of accumulating and commu-

nicating electricity, is proved by the well known experiment of taking the two hind legs of a vivaceous frog, immediately after they are cut off, laying bare the crural nerves, applying one of these to the exposed muscles of the other limb, and then when the circle is completed by raising the other crural nerve with a glass rod, and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs. The ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles, and the course of the nerves of the viscera which keep up a connexion among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chemistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

Experiment 1. Middle of January, 1809.

The conductors from twenty four four inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed, as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire, was of a deep red colour and extremely alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

Experiment 2. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of

an hour, a third ligature was then tied in the centre of the detached vein, in order to cut off the connection between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long continued electrical action, the serum only was employed in the following experiments.

Experiment 3. 10th of March, 1809.

The conductors from one hundred and twenty four inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved: at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

Experiment 4. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power; a battery was employed, consisting of twelve four inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole; in five minutes, the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in

length was seen floating in the fluid, and adhering to the same wire.

Experiment 5. 6th of May, 1809.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three inch plates *very weakly* charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, (super salts.)

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form, with a less degree, it is separated in a fluid form.

From these facts, the following queries are proposed.

1st. That such decomposition of the blood by electricity, may be as near an approach to secretion, as could be expected to be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from

the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose, and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. HATCHETT's experiments, albumen is the principal material of which animal bodies are composed.

5. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6. As albumen becomes visibly coagulated, by the effect produced from twelve four inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chemical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the Society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

XXIII. *On the comparative Influence of Male and Female Parents on their Offspring.* By Thomas Andrew Knight, *Esq. F. R. S.* *In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read June 22, 1809.

My Dear Sir,

I have been engaged, during many years, in experiments on fruit-trees, of which the object has been to discover the best means of forming new varieties, that may be found better calculated for the climate of Britain than those at present cultivated. In this inquiry my efforts have been always most successful, when I propagated from the males of one variety and the females of another; and I was enabled, by the same means, to ascertain more accurately, than had previously been done, the comparative influence of the male and female parent on the character of the offspring. The analogy that subsists between plants and animals, in almost every thing which respects generation, induced me also to attend very minutely to similar experiments in which I engaged on some species of animals; and as the repetition of such experiments would necessarily require a very considerable space of time, and as the results seem to lead to conclusions that may be of public utility, I have thought the following account sufficiently interesting to induce me to address it to you.

Linnæus conceived, that the character of the male parent predominated in the exterior parts both of plants and animals;

and the same opinions have been generally entertained by more modern naturalists. But the Swedish philosopher appears to have been misled, by the striking predominance of the character of the male parent in male animals, and to have drawn his conclusions somewhat too generally: for I have observed that seedling plants, when propagated from male and female parents of distinct characters and permanent habits, generally, though with some few exceptions, inherit much more of character of the female, than of the male parent, and the same remark is applicable, in some respects, to the animal world, as I shall point out in the succeeding narrative.

My experiments were made on many different species of fruit-trees; but most extensively, and under the most advantageous circumstances, on the apple-tree; and as the results were all in unison with each other, it will be necessary to trouble you only with an account of some of the experiments which were made on that species of fruit-tree.

The apple, or crab of England, and of Siberia, however dissimilar in habit and character, appear to constitute a single species only; in which much variation has been effected by the influence of climate on successive generations: for the two varieties readily bred together, and the offspring, whether raised from the seeds of the Siberian, or British variety, were prolific to a most exuberant extent. But there was a very considerable degree of dissimilarity in the appearance of the offspring; and the leaves, and general habits of each, presented an obvious prevalence of the character of the female parent. The buds of those plants, which had sprung from the seeds of the cultivated apple, did not unfold quite so early in the spring; and their fruits generally exceeded, very con-

siderably, in size those which were produced by the trees which derived their existence from the seeds of the Siberian crab. There was also a prevalence of the character of the female parent in the form of the fruit; but the same degree of prevalence did not extend to the quality and flavour of the fruit; for the richest apple that I have ever seen, and which afforded expressed juice of much higher specific gravity than any other, sprang from a seed of yellow Siberian crab.

The prevalence of the character of the female parent in the preceding cases, may possibly be suspected to have arisen from some error, or neglect of accuracy in making the experiments; but I do not conceive that any such errors could have existed; for the trees of each variety were trained to walls, where they blossomed much before any others of the same species, and the stamina were always carefully extracted, whilst immature, from every blossom, which I intended to afford seeds. The remaining blossoms of the trees were also totally destroyed, and no other blossoms, except those from which the pollen was taken, were ever unfolded in the neighbourhood, in the season when the experiments were made; and I have also invariably declined to draw any conclusion from the appearance of a plant, in which I could not certainly distinguish some portion of the features and character of the supposed male parent.

It is perhaps also proper to state, that the predominance of the character of the female parent, could scarcely have arisen from any defective action of the pollen; for, except in cases where superfœtation took place, I have invariably found the effect of a very large, or a very small quantity of pollen, to be invariably the same, in its influence on the offspring; and

in the greater part of the experiments, from which I have drawn the preceding conclusions, more than ten times as much pollen was deposited on the stigmata, as could have been deposited in unmutilated blossoms by the ordinary means employed by nature.

In all attempts to discriminate the different influence of the male and female parent on the offspring of animals many difficulties present themselves, owing to the intermixtures which have been made of the different breeds of domesticated animals of every species, and the consequent absence of all hereditary permanency in the character of each variety. For under these circumstances, the offspring will be very frequently found to shew little resemblance either to its male or female parent, either in form, or stature, or colour. It will therefore be necessary, before I enter on the subject of viviparous animals, to observe that when I apply the terms large and small to the male or female parent, I extend the meaning of those terms to the parentage, from which the male and female descend, and not to the size of the individual only, which becomes the immediate parent of the offspring.

Mr. CLINE has observed, in a communication to the Board of Agriculture, that if the male and female parent differ considerably in size, the dimensions of the fœtus, at the birth, will be regulated much more by the size of the female than of the male parent; and, if the meaning of the terms large and small be extended to the varieties, as well as to the individuals, his remark is perfectly just. But experience compels me wholly to reject the inference that he has drawn respecting the advantages of propagating from large, in preference to small females.

Nature has given to the offspring of many animals (those of the sheep, the cow, and the mare, afford familiar examples) the power, at an early age, to accompany their parents in flight; and the legs of such animals are very nearly of the same length, at the birth, as when they have attained their perfect growth. When the female parent is large, and the fœtus consequently so, the offspring will be large at its birth, in proportion to the bulk it will ultimately attain, and its legs will thence be long comparatively with the depth of the chest and shoulders. When, on the contrary, the female is small, and the fœtus so, at the birth, the length of the legs of the young animal will be short comparatively with the depth of its chest and shoulders; and an animal in the latter form will be greatly preferable, either for the purposes of labour, or of food to mankind. I have seen this difference in the influence of the male and female parent, on the offspring, very strikingly exemplified, in the result of an attempt to obtain very large mules from the male ass and the mare. The largest females, that could be procured, were selected, and the forms of the offspring, at the birth, were perfectly consistent with the theory of Mr. CLINE; they were remarkably large: and I observed, that the length of their legs, when they were only a few days old, very nearly equalled that of the legs of their female parents. I examined the same animals when five years old, and in the depth of their chests and shoulders, they very little exceeded their male parent; and they were consequently of little or no value; whilst other mules, which were obtained from the same male parent (a Spanish ass), but from mares of small stature, were perfectly well proportioned. I have never seen the little mule, which is propagated from the

female ass and the horse, nor even a delineation, or description of its form; but I do not entertain any doubt that its chest and shoulders are excessively deep and strong, comparatively with the length of its legs, and that, on account of this peculiarity in its form, it has been so frequently shewn on the Continent, under the name of a jumart, as the pretended offspring of the mare and the bull.

In opposing the theory advanced by Mr. CLINE, it is not by any means my intention to enter the lists with him, as a physiologist; but, as a farmer and breeder of animals of different species, I have probably had many advantages, which he has not possessed; and my conclusions have been drawn from very extensive, and, I believe, accurate observation.

There is another respect in which the powers of the female appear to be prevalent in their influence on the offspring, and that is relative to its sex. In several species of domesticated, or cultivated animal (I believe in all), particular females are found to produce a very large majority, and sometimes all their offspring of the same sex; and I have proved repeatedly, that, by dividing a herd of thirty cows into three equal parts, I could calculate, with confidence, upon a large majority of females from one part, of males from another, and upon nearly an equal number of males and females from the remainder. I frequently endeavoured to change these habits by changing the male; but always without success; and I have in some instances observed the offspring of one sex, though obtained from different males, to exceed those of the other, in the proportion of five or six, and even seven to one. When, on the contrary, I have attended to the numerous offspring of a single bull, or ram, or horse, I have never seen

any considerable difference in the number of offspring of either sex. I am therefore disposed to believe that the sex of the offspring is given by the female parent; and the probability of this seems obvious in fishes, and several other species of animals which breed in water; and though the evidence afforded by the facts adduced is not by any means of sufficient weight to decide the question, it probably much exceeds all that can be placed in the opposite scale.

In oviparous animals, I have had reason to think the influence of the female parent quite as great, as amongst the viviparous tribes, though my observations have been more limited, and less conclusive. In viviparous animals, the size of the fœtus is affected by the influence of the male parent, and, in some instances, not inconsiderably; but the size and form of the eggs of birds do not appear to be in any degree changed or modified, by the influence of the male; and therefore the size of the offspring, at the birth, must be regulated wholly by the female parent; and this circumstance permanently affects the form and character of the offspring. The eggs of birds, and those of fishes and insects (if such can properly be called eggs), appear to resemble the seeds of plants, in having their forms and bulk wholly regulated by the female parent; but nevertheless their formation appears to depend on very different laws. For the eggs, both of birds and of fishes and insects, attain their perfect size in total independence of the male, and the cicatricula, the vitellus, and the chalazæ have appeared (I believe) to the most accurate observers, to be as well organised in the unimpregnated, as in the impregnated egg: in the seed, on the contrary, every thing relative to its internal organisation appears dependent on the male parent. SPALLANZANI has,

however, stated, that many plants produced well organised seeds, and even seeds which vegetated perfectly, under circumstances in which it is not easy to conceive how the pollen of the male plant or flower could have been present. But the Italian naturalist appears to have blundered most egregiously in his experiment; or (which I conceive to be more probable) he became the dupe of the refined malice of his countrymen; for, I repeated his experiments under very favourable circumstances, and with the closest attention, but I failed to obtain a single seed. The gourd alone produced apparently perfect fruit, and the seed-coats acquired their natural size and form; and in this respect the growth of its seeds appeared to be, like that of eggs, wholly independent of the influence of the male. But the *seed-coats* of the gourd were perfectly *empty*, and I could not discover, at any period of their growth, the slightest vestige either of cotyledons, or plumule, nor of any thing that appeared to correspond with internal organisation of a seed of the same plant, under different circumstances. SPALLANZANI has not, I believe, mentioned the species of gourd upon which he made his experiments: the common, or orange gourd of our gardens, was the subject of mine.

In comparing the mode of the formation and growth of eggs with the observations I had previously made on the growth of seeds, I have been favoured with the very able assistance of Mr. CARLISLE, for which I have on this, as on many other occasions, to acknowledge much obligation.

I am, my Dear Sir,

with great respect, sincerely yours,

THOMAS AND. KNIGHT.

Downton, May 20, 1809.

XXIV. *On the Effect of westerly Winds in raising the Level of the British Channel. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S. By* James Rennell, *Esq. F. R. S.*

Read June 22, 1809.

Dear Sir,

In the "*Observations on a Current that often prevails to the "Westward of Scilly*," which I had the honour to lay before the Royal Society many years ago, I slightly mentioned, as connected with the same subject, the effect of strong westerly winds, in *raising* the level of the British Channel; and the escape of the super-incumbent waters, through the Strait of Dover, into the *then* lower level of the North Sea.

The recent loss of the Britannia East India ship, Captain Birch, on the Goodwin Sands, has impressed this fact more strongly on my mind; as I have no doubt that her loss was occasioned by a current, produced by the running off of the accumulated waters; a violent gale from the westward then prevailing. The circumstances under which she was lost, were generally these:

In January last she sailed from her anchorage between Dover and the South Foreland (on her way to Portsmouth), and was soon after assailed by a violent gale between the west and south-west. The thick weather preventing a view of the *lights*, the pilot was left entirely to the reckoning and the lead; and when it was concluded that the ship was quite clear of

the Goodwin, she struck on the north-eastern extremity of the southernmost of those sands. And this difference between the reckoning (after due allowance being made for the tides) and the actual position, I conclude was owing to the northerly stream of current, which caught the ship when she *drifted* to the *back*, or *eastern side* of the Goodwin.

The fact of the high level of the Channel, during strong winds, between the W. and SW., cannot be doubted: because the increased height of the tides in the southern ports, at such times, is obvious to every discerning eye. Indeed, the form of the upper part of the Channel, in particular, is such as to receive and retain, for a time, the principal part of the water forced in; as may be seen by the sketch (No. 2): and as a part of this water is continually escaping by the Strait of Dover, it will produce a current; which must greatly disturb the reckonings of such ships as navigate the Strait, when thick weather prevents the land, or the lights of the Forelands, and the North Goodwin, from being seen.

I observe in a new publication of Messrs. LAWRIE and WHITTLE, entitled "*Sailing Directions, &c. for the British Chan-*" *nel*, 1808," that throughout the Channel, it is admitted by the experienced persons whom he quotes, that strong SW. winds "cause the flood tide to run an hour, or more, longer, "than at common times:" or in other words, that *a current overcomes the ebb tide, a full hour*: not to mention how much it may accelerate the one, and retard the other, during the remainder of the time.*

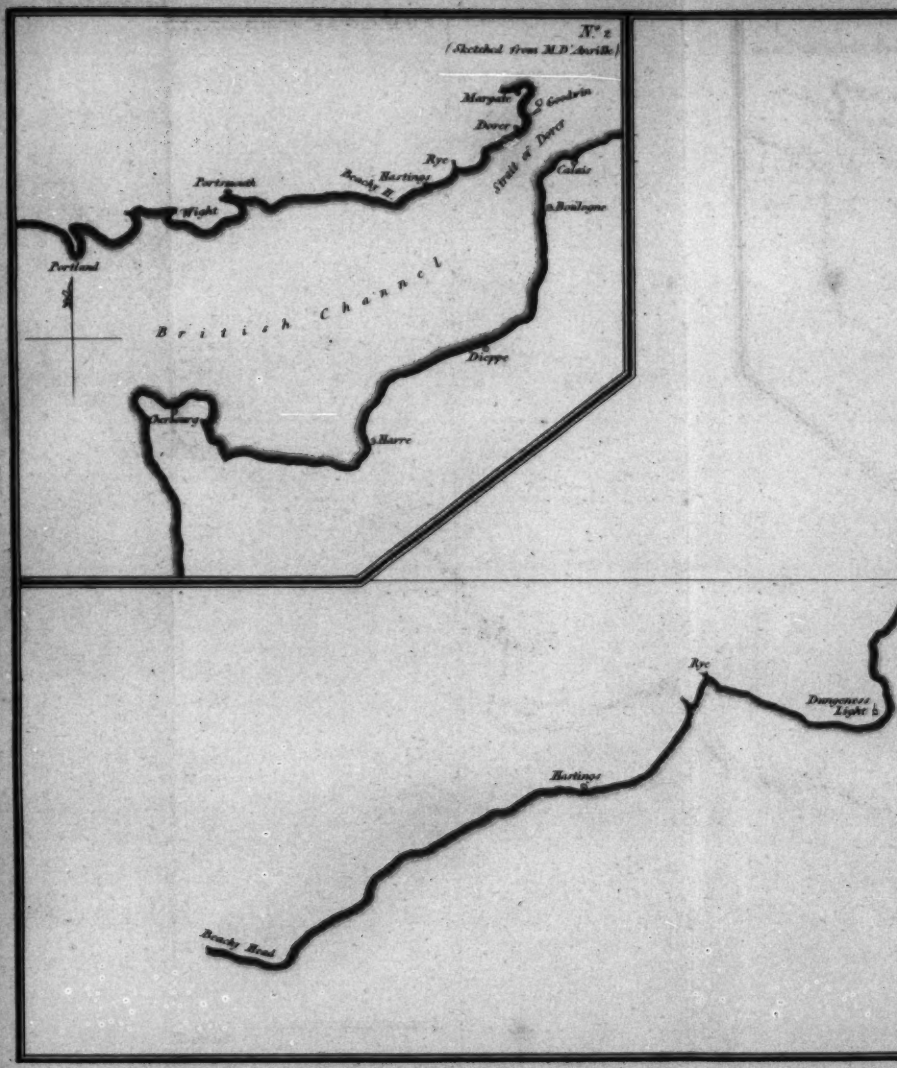
* It is also asserted, that in the mouth of the Channel, the extraordinary rise of tide, in stormy weather, is ten feet: that is, at common springs twenty, and in storms thirty feet. See pages 28, 41, 70, and 133.

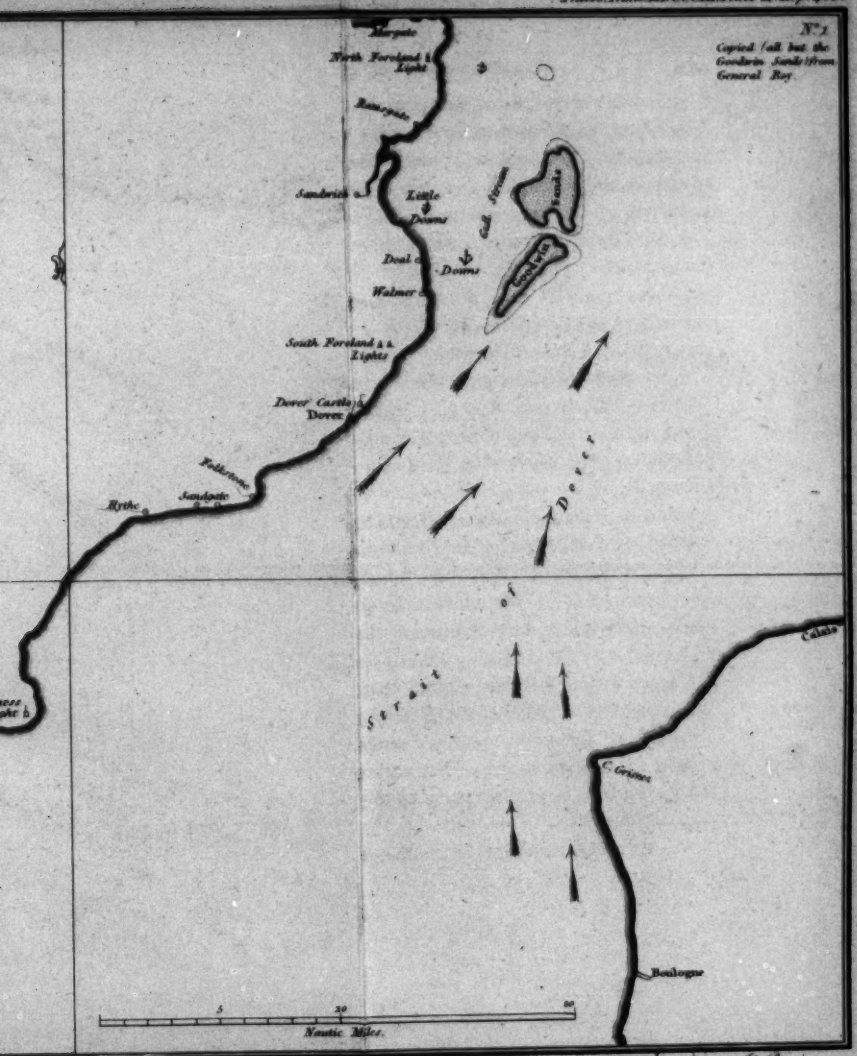
It is evident, that the direction of the current under consideration, will be influenced by the form and position of the opposite shores, at the entrance of the Strait; and as these are materially different, so must the direction of the stream, be, within the influence of each side, respectively. For instance, on the English side, the current having taken the direction of the shore, between *Dungeness* and the *South Foreland*, will *set* generally to the north-east, through *that* side of the Strait. (See No. 1.) But, on the French side, circumstances must be very different: for the shore of *Boulogne* trending almost due north, will give the current a like direction, since it cannot turn sharp round the Point of *Grisnez*, to the north-eastward; but must preserve a great proportion of its northerly course, until it mixes with the waters of the North Sea. And it may be remarked, that the Britannia, when driven to the eastward of the Goodwin, would fall into this very line of current.

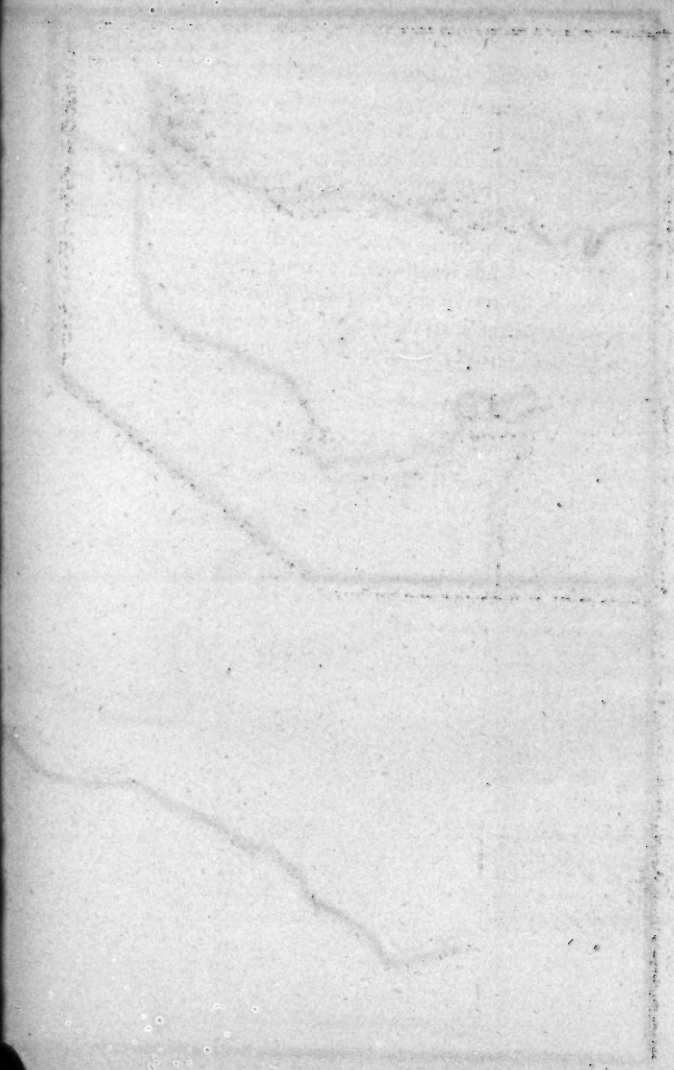
There is another circumstance to be taken into the account; which is, that the *shore of Boulogne* presenting a direct obstacle to the water impelled by the westerly winds, will occasion a higher level of the sea, there, than elsewhere; and, of course a stronger line of current towards the Goodwin. (See again No. 2.)

It must, therefore, be inferred, that a ship, passing the Strait of Dover, at the back of the Goodwin Sands, during the prevalence of strong W. or SW. winds, will be carried many miles to the northward of her reckoning; and if compelled to depend on it, may be subject to great hazard, from the Goodwin.

It will be understood, of course, that although the stream


N.° 2
(Sketched from M.D'Anville)
Margate
Goodwin
Dover
Rye
Hastings
Beachy H.
Strait of Dover
Calais
Boulogne
Portsmouth
Wight
Portland
British Channel
Dieppe
Jarsey
S.Marre
Rye
Dungeness Light h
Hastings
Beachy Head


N°. 1
Copied (all but the Goodwin Sands) from General Roy.
Margate
North Foreland Light
Ramsgate
Sandwich
Little Downs
Gull Stream
Sands
Goodwin
Deal
Downs
Walmer
South Foreland Light
Dover Castle
Dover
Folkestone
Rythe
Sandgate
Dover
Strait of
Calais
C. Grisnez
Boulogne
5
10
20
Nautic Miles.
J. Basire sc.



of current, alone, has been considered here (in order to simplify the subject), yet that, in the application of these remarks, the regular tides must also be taken into the account. But from my ignorance of their detail, I can say no more than that I conceive that the great body of the tide from the Channel, must be subject to much the same laws, as the current itself. The opposite tide will doubtless occasion various inflexions of the current, as it blends itself with it; or may absolutely suspend it: and the subject can never be perfectly understood, without a particular attention to the velocity and direction of the tides in moderate weather, to serve as a ground-work.*

I am, with great respect,

Dear Sir,

your faithful humble servant,

J. RENNELL.

* Messrs. LAWRIE and WHITTLE's publication, allows the tides in this quarter a velocity of one mile and a half per hour, at the springs; half a mile at the neaps. The Britannia's ordeal happened at dead neaps.

XXV. *On Respiration.* By William Allen, *Esq. F. R. S. and* William Hasledine Pepys, *Esq. F. R. S.*

Read June 22, 1809.

One of the most prominent features in our last communication was the evolution of a considerable quantity of azote, when oxygen gas nearly pure was respired; and although a considerable part of this azote must undoubtedly be attributed to the residual gas in the lungs, after the most forcible attempt at expiration, yet the fact seemed to demand still farther investigation, it appearing of consequence to ascertain whether the increase of azote was uniform throughout the latter stages of the experiment, or *solely* confined to the earlier periods.

By adverting to our former Paper, it will be found, that in an experiment where more than 3000 cubic inches of oxygen passed through the lungs in seven minutes and a quarter, 69 cubic inches of azote were found in the first 250 cubic inches expired, though the gas originally contained but 2,5 per cent. or only 6 cubic inches in this quantity; in the two next portions expired, consisting of 562 cubic inches, we found 56 cubic inches of azote, though this quantity of gas, before it was respired, contained only 14; these, first portions, were given off in about two minutes, and contained nearly 100 cubic inches of azote more than could be accounted for in the oxygen employed; hence it is plain, that a large proportion of the increase is evolved in the first periods of the process.

Our attention was particularly directed to this point in the following experiment. The oxygen, procured as usual from hyperoxygenised muriate of potash, was found to contain four per cent. of azote; the experiment was conducted in the same manner as the preceding ones, except that the tubes of the gasometers were filled with oxygen, and the gas was not merely passed *once* through the lungs, but breathed backwards and forwards. In order to prolong the duration of the experiment, which began and ended with a forcible expiration, portions of the respired gas were preserved for examination from each of the gasometers, in the following order:

No. 1.	244
2.	294
3.	282
4.	266
5.	230
6.	266
7.	254
8.	288
9.	252
10.	168
	2544

The portion of oxygene remaining in the water gasometer of the original quantity, not employed in the experiment, was found upon trial to contain four per cent. of azote, as before.

Summary of the Experiment.

Bar.	Therm.	Cub. Inches of Oxygen inspired.	Cub. Inches of Gas expired.	Deficiency.	Time.
29.9	51	2668	2544	124	13 minutes:

here the deficiency was greater than we had ever remarked before; but on passing an equal quantity of common air from the water gasometer, and registering it in the mercurial ones, we were satisfied that the apparatus was quite perfect. It is, however, to be considered, that the respiration in this case was not natural, and that some small degree of force was required when the inspirations and expirations were made in the mercurial gasometers, which renders this experiment rather different from those which had preceded it; and it appears to us probable, that a portion of air was forced into the extremities of the bronchea, which could not be suddenly expelled by the strongest attempts at expiration. Hence also, perhaps, the constant though smaller deficiency, even when the air was only once passed through the lungs; but when the process is continued for a much longer time, it is probable that the vessels recover their tone, and are able to expel nearly the whole of the volume admitted.

The air expired in the present instance, being examined in the manner described in our last paper, we found that 100 parts from each of the gasometers contained the following proportions:

No. 1.	10 carbonic acid
	21 azote
	69 oxygen
	100
No. 2.	10 carbonic acid
	11 azote
	79 oxygen
	100

No. 3.	10	carbonic acid
	8,5	azote
	81,5	oxygen
	100	
No. 4.	10	carbonic acid
	7,75	azote
	82,25	oxygen
	100	
No. 5.	10	carbonic acid
	7	azote
	83	oxygen
	100	
No. 6 to 10 mixed	10,5	carbonic acid
	5,5	azote
	84	oxygen
	100	

we shall first calculate the total quantity of azote existing in the gas before the experiment, and afterwards estimate what was produced in the different periods during the first half of the experiment.

Calculation for Azote.

2668 cubic inches of oxygen were employed containing four per cent. azote: then

$$100 : 4 :: 2668 : 106{,}72$$

the total quantity of azote in the gas consumed, was 106,72 cubic inches.

Azote found after the Experiments.

	Cubic Inches.			Azote found.
No. 1.	244	100 : 21	:: 244 :	51,24
2.	294	100 : 11	:: 294 :	32,34
3.	282	100 : 8,5	:: 282 :	23,97
4.	266	100 : 7,75	:: 266 :	20,61
5.	230	100 : 7	:: 230 :	16,10
6 to 10.	1228	100 : 5,5	:: 1228 :	67,54
			Total	211,80 cubic inches.

The whole azote, found after the experiment, was - - - 211,80 cubic inches,
Azote detected by the same tests before the experiment only - - 106,72

Increase of azote 105,08

Now, as the whole time was thirteen minutes, if we divide this by the number of gasometers filled, it will give us one minute eighteen seconds for each, and the following will be the periods in which the azote was evolved.

	Time.	Azote found.		Azote in the Oxygen.		Increase.
	′ ″					
No. 1.	1.18	51,24	less	9,76	equal to	41,48
2.	1.18	32,34	—	11,76	=	20,58
3.	1.18	23,97	—	11,28	=	12,69
4.	1.18	20,61	—	10,64	=	9,97
5.	1.18	16,10	—	9,20	=	6,90
6 to 10.	6.30	67,54	—	49,12	=	18,42
	13 min.	211,80		101,76		110,04

Here the increase of azote appears rather greater, *viz.* 110 cubic inches, but the calculation in this case is made upon the gas *expired*, and, from the above statement, we may see, that the evolution of azote goes on diminishing; we have sometimes even found, that towards the close of an experiment it has been almost reduced to nothing. The question now is, whether this increase of azote can be owing to the residual gas contained in the lungs at the beginning of the experiment, or whether a portion of oxygen is not actually exchanged for azote, when pure oxygen gas is respired.

Here it may be useful to compare the azote found in our former experiments on oxygen, with the present.

	Bar.	Therm.	Oxygen Gas respired.	Gas expired.	Deficiency.	Time.	Quantity respired in a Minute.	Azote evolved.	Inferred Capacity of Lungs.
		°				′ ″			
No. 1.		53	3260	3193	67	9,20	348	110	141
2.	30,3	70	3420	3362	58	7,25	461	177	225
3.	30,15	70	3130	3060	70	8,45	357	187	236
4.	29,9	51	2668	2544	124	13,	205	165	133

The greatest increase of azote was in the 2d and 3d experiments, when the thermometer was at 70°, which might materially influence the results: in the other cases, it was not higher than 53.

From the experiments of GOODWIN, we might be inclined to admit the capacity of the lungs, inferred from the 1st and 4th experiments, as very possible; but it seems difficult to conceive that it can amount to 236 or 225 cubic inches, and yet this must be the case, unless a portion of azote is given off from the blood, or there is some process in nature by which it is capable of being produced from oxygen.

Having, by the kindness of our friend HENRY CLINE, jun.

been furnished with the lungs of a stout man, about five feet ten inches high, taken from the body not long after death, and in a sound state, we proceeded to ascertain the quantity of air contained in this organ after the most complete expiration, as in death.

HENRY CLINE had judiciously taken the precaution to divide the trachea just below the cricoid cartilage, before he opened the thorax; he then inserted a tube with a brass stop-cock, which he tied firmly to the trachea, and attached an empty bladder to the other end. The cock was then turned, so as to communicate with the bladder, and on opening the thorax 31½ cubic inches of air were expelled into it. The weight of the lungs was four pounds one ounce. A very large glass jar being placed in a shallow tin vessel, was filled to the brim with water, the lungs were then completely immersed, and the water which flowed over, and was the measure of their volume, weighed six pounds two ounces; we next cut a portion of the lungs into small pieces, under a large inverted glass of water, and attempted to squeeze the air from the cells, but although several cubic inches were thus procured, we were soon convinced that it was utterly impossible to arrive at our object by these means, as no force that we could use seemed capable of expelling the air from the cellular membrane, into which it escaped from the vesicles. We therefore took portions of the lungs, which weighed 2774 grains; the mass being put into a piece of new hair cloth, was subjected to the action of a powerful screw press, and the fluid was received in a vessel; after twice undergoing this operation, the mass weighed only 660 grains. Its specific gravity was very nearly that of water, *viz.* .930 water being 1,000: the fluid procured

by the press, was of the specific gravity of 1,019; this would make the specific gravity of the lungs .997, water being 1,000; hence it appears, that the substance of the lungs, and the contents of the blood-vessels together, are so near the specific gravity of water, that they may be fairly considered as the same.

Then, as the mass of the lungs was equal to 4 pounds of water, though 6,2 pounds of water were displaced by them, and as a pound of water occupies the space of 28,875 cubic inches, we have the following calculation:

lbs.	oz.	
6	2	water displaced by the lungs
4	1	weight of the lungs
2	1, or 59,554	cubic inches of air in the lungs, to which must be added 31,580 the volume of the air forced into the bladder on opening the thorax.
		91,134

and this gives us 91,134 cubic inches, as the air contained in the lungs of this person after death; and when we reflect that the air must have been under compression, when the lungs were immersed in water, some force being required to keep them down, and also that not less than 7 or 8 cubic inches must be contained in fauces, &c., we cannot estimate the whole at less than 100 cubic inches.

It is farther to be noted, that these 100 cubic inches would occupy much more space in the temperature of the human body, than in the mean temperature in which the examination was made; and this difference would be nearly 8 cubic inches; the air left in the lungs, after complete expiration, would

therefore be 168 cubic inches; but the mean of our experiments would make it 183.

Experiment 1.	141
2.	225
3.	236
4.	133
	4)735
	183

we are then almost compelled to allow that when pure oxygen is respired, a portion of azote is given off from the blood.

We now resolved to perform a series of experiments upon some animal which lived wholly upon vegetable food, and made choice of the Guinea pig as one of the most manageable.

The apparatus consisted of our two large mercurial gasometers, which were made to communicate with a strong trough E, in the middle of which a small mahogany table D was made fast by a screw, for the purpose of supporting the animal under the bell-glass A, two holes were made through the table for the insertion of tubes to supply, and take off the air, each of them communicated with one of the mercurial gasometers; the tube B delivered gas towards the upper part of the glass A, in order to bring the supply of fresh air near the head of the animal: the opening of the tube C was placed within half an inch of the table to convey off the respired air; the gasometer connected with this tube, was made to communicate with a mercurial bath G, in which portions of the respired air were preserved for examination. Quicksilver being

poured into the trough E, so as to rise to a level with the top of the mahogany stand, we placed a Guinea pig upon it, with the bell-glass over him, and as its edges were immersed in quicksilver, the animal was completely confined in atmospheric air: we found that his body occupied the space of 89 cubic inches, which deducted from the cubic contents of the glass A, left 55 cubic inches for the air confined with the pig, to which must be added 5 more for that contained in the tube C.

First Experiment with Atmospheric Air.

The pig was placed upon the stand, and the apparatus arranged as represented in the plate: 250 cubic inches of atmospheric air were admitted into the mercurial gasometer communicating with B: the gasometer communicating with C was quite empty, the apparatus being tried was found perfectly air tight, and the whole quantity of air 310 cubic inches.

The cocks H and I being opened, gentle pressure was made upon the glass of gasometer B, so as to cause the air to pass through A, which consequently drove an equal portion through the tube C into the empty gasometer; a quarter of an hour was employed in passing the gas, which measured exactly 250 cubic inches in C, so that there was no alteration of volume; the cocks H and I were now closed, and the respired air being examined by the usual methods, 100 parts were found to contain

5 carbonic acid
16 oxygen
79 azote
100

As the air after the experiment had experienced no alteration of volume, and as it contained the same proportion of azote as atmospheric air, this substance had remained unaltered. But 15,50 cubic inches of oxygen had been converted into carbonic acid gas.

100 : 5 : : 310 : 15,50.

Summary of the Experiment.

Bar.	Therm.	Atmos. air Inspired.	Gas after experiment.	Cub. inches of carb. acid.	Cub. in. of carb. acid per minute.	Time.
30°	48°	310	310	15,5	,62	25 min.

Experiment II. *Atmospheric Air.*

The experiment was repeated in exactly the same manner; the animal, except from confinement, appeared much at his ease all the time. The air after the experiment contained in 100 parts

5,5 carbonic acid
15,5 oxygen
79 azote

100

here the proportions of azote were undisturbed, and 17,05 cubic inches of carbonic acid procured

100 : 5,5 : : :310 : : 17,05

Summary of the Experiment.

Bar.	Therm.	Atmos. air Inspired.	Air after Experiment.	Carb. acid found.	Carb. acid per minute,	Time.
29,66	38°.	310.	310	17,05	,68	25 min.

Experiment III. *Atmospheric Air.*

The apparatus being arranged as before, we kept the pig in the glass A for one hour, and during that time passed 1000

cubic inches of atmospheric air through it, which measured 1061: portions of the respired gas had been preserved in the mercurial bath, and the usual trials made upon the mixture, which was found to contain 5 parts of carbonic acid in every 100, or 53 cubic inches in the whole quantity; the azote was unaltered; 100 : 5 :: 1060 : 53.

Summary of the Experiment.

Barom.	Therm.	Atmos. air before expt.	Air after expt.	Increase.	Carb. acid found.	Carb. acid per minute.	Time.
29,8	56°	1060	1061	1	53	,88	1 hr.

Experiment IV. *Oxygen Gas.*

The pig hitherto employed was put into the glass vessel A, which with the tube contained 60 cubic inches of atmospheric air; 250 cubic inches of oxygen, containing 3 per cent. of azote, were admitted into the gasometer communicating with B, and during a quarter of an hour were made to pass slowly through the vessel in which the animal was confined, to the empty gasometer communicating with C, where it measured exactly 250 cubic inches; a portion was preserved in the mercurial bath for examination, and the gasometer B was replenished with 250 cubic inches of the same oxygen; this was passed in about the same time as before, through A into gasometer C, when it measured 248 cubic inches.

250 cubic inches more of the oxygen were now admitted into gasometer B, and passed in the same manner through A into C, where they measured 249.

The gasometer B was for the fourth and last time supplied with 250 cubic inches more of the oxygen, which were passed as before, through A into C, during about a quarter of an hour, and then measured 249.

The pig had remained in the vessel one hour and twelve minutes; it did not appear to have suffered in the least; portions of the respired gas were saved from each of the gasometers, and examined as usual.

Cubic Inches.	Contained in 100 parts.		Carb. Acid.	Oxygen.	Azote.
No. 1. 250	Carb. acid	8	20		
	Oxygen	66		165	
	Azote	26			65
		100			
No. 2. 248	Carb. acid	10	24,80		
	Oxygen	78		193,44	
	Azote	12			29,76
		100			
No. 3. 249	Carb. acid	10	24,90		
	Oxygen	80		199,20	
	Azote	10			24,90
		100			
No. 4. 249	Carb. acid	12	29,88		
	Oxygen	79		196,71	
	Azote	9			22,41
		100			
In Glass A. and tube C. 60	Carb. acid	12	7,20		
	Oxygen	79		47,40	
	Azote	9			5,45
1056		100	106,78	801,75	147,52

Total, gas before experiment, 1060

after 1056

Deficiency, 4

Calculation for Oxygen.

We do not calculate upon the tube from gasometer B, because it is always in the same state after the experiment as before.

1000 cubic inches of oxygen containing 5 per cent. azote, consisted of	950	pure oxygen
60 Atmospheric air with the pig, and in tube C, containing 21 per cent. oxygen	12,60	
Total, oxygen before experiment,	962,60	
Oxygen found after experiment,	801,75	
Ditto in carbonic acid -	106,78	
	908,53	
Oxygen missing,	54,07	

Calculation for Azote.

1000 cubic inches containing 5 per cent. azote - - -	50	
60 Atmospheric air, containing 79 per cent.	47,40	
Total azote before experiment,	97,40	
Ditto found after experiment,	147,52	
Increase of azote,	50,12	

This increase of azote was much more than equal to the cubic contents of the animal's body, the deficiency of 4 cubic inches was doubtless oxygen absorbed.

Summary of the Experiment.

Bar.	Therm.	Oxygen, &c. inspired.	Gas after experiment.	Deficiency.	Carb. acid found.	Carb. acid per min.	Time.	Oxygen missing.	Azote added.
29,05	57°	1060	1056	4	106	1,48	1 h. 12 m.	54,07	50,12

Experiment V. *Oxygen.*

In this experiment we employed a smaller pig, which occupied the space of 33 cubic inches, and our object was to keep him for the same length of time in a smaller quantity of gas, we therefore only used 750 cubic inches of oxygen, beside the common air contained in the glass A, and tube, amounting to 66 cubic inches; the first 250 cubic inches were passed through the glass A into C in 24 minutes, where it appeared to have undergone no alteration of volume. 250 cubic inches more were passed during the next 23 minutes, and these measured 248 in C; the last 250 were passed in 24 minutes, and the volume remained unaltered. The animal did not appear to suffer the slightest inconvenience, except from the confinement.

State of the Gas before Respiration.

		Oxygen.	Azote.
66 cubic inches of atmospheric air,	=	13,86	52,14
750 oxygen, containing 5 per cent. azote,	=	712,50	37,50
816 total consisting of		726,36	89,64

The oxygen was tried before, as well as after the experiment, and both the results agreed perfectly with each other. We now examined portions of gas preserved from the three gasometers, with lime water, and the tests for oxygen.

	Time. min.	Contained in 100 parts.		Carbonic acid.	Oxygen.	Azote.
No. 1.	250.	24	Carb. acid	9.5	28,75	
			Oxygen,	60,5	151,25	
			Azote,	30		75
				100		
No. 2.	248.	23	Carb. acid,	9.5	23,56	
			Oxygen,	81	200,88	
			Azote,	9.5		23,56
				100		
No. 3.	250.	24	Carb. acid,	10	25	
			Oxygen,	82	205	
			Azote	8		20
				100		
66 with pig, as No. 3.					6,60	54,12	5,28
814	71				78,91	611,25	123,84

Calculation for Oxygen.

Oxygen before the experiment -	726,36
Ditto after - - -	611,25
In carbonic acid - -	78,91
	690,16
Loss of oxygen	36,20

Calculation for Azote.

Azote found after experiment - -	123,84
Ditto before experiment - -	89,64
Increase of azote	34,20

Summary of the Experiment.

Therm.	Gas before Exper.	Gas after Exper.	Deficiency.	Carbonic Acid found.	Cubic Inches per Minute.	Time. h	m	Oxygen missing.	Azot added.
56	816	814	2	78,91	1,11	1	11	36,20	34,20

The quantity of azote added, of oxygen missing, and of carbonic acid formed, were smaller than in the last experiment; but the animal in this instance was smaller, as well as the quantity of oxygen passed through in a given time.

In this case, as in the human subject, the increase of azote takes place principally in the early periods. The whole azote contained in the 66 cubic inches, confined with the pig, was only 52,14, but supposing, which perhaps was not the case, that the 66 of common air were expelled by the first 250 cubic inches of oxygen, we should have 250

less 66

184 of oxygen,

containing 5 per cent. azote, or 9,20 cubic inches; these added to the 52,14, would make 61,34 of azote to be found in the first gasometer of respired gas, but we detected 75, so that even on this supposition 13,66 of azote were added in the first twenty-four minutes.

The azote contained in the second gasometer before respiration, was 12,50 cubic inches, but after it had been respired for twenty-three minutes, we found 23,75, or an increase of 11,25 azote. The azote contained in the third gasometer, before respiration, was, as before, 12,50 cubic inches; but after it had been respired for twenty-four minutes, we found 20, or an increase of 7,50 azote.

The azote contained in the 66 cubic inches, was 3,30, but we found 5,28, or an increase of 1,98 azote.

From the results of these experiments, it seemed that when the *usual proportion* of azote was not present in the gas respired, there was a disposition in the blood to give out a certain quantity in exchange for an equal volume of oxygen, and we resolved to try, whether this circumstance would occur when hydrogen was substituted for azote, we accordingly made a mixture containing 22 per cent. oxygen and 78 hydrogen.

Experiment 6. *Hydrogen and Oxygen.*

The pig employed in the last experiment, was placed upon the stand in the glass A, with 66 cubic inches of common air as usual.

250 cubic inches of the mixture were passed from the gasometer, communicating with B through the glass A into the gasometer communicating with C during sixteen minutes. The animal did not appear uneasy: a second quantity of 250 cubic inches was passed in seventeen minutes and three quarters: the animal did not seem to be in the least incommoded.

A third quantity of 250 cubic inches was passed, in about sixteen minutes.

And a fourth quantity of 250 cubic inches in eleven minutes and three quarters; but during this time, the animal became very sleepy, and towards the end of the experiment, kept his eyes constantly shut; he, however, appeared to suffer nothing, and was easily roused for a short time by rapping at the side of the glass. At the end of sixty one minutes and a half, he was taken out, and we found that during this time, he had produced 60,20 cubic inches of carbonic acid gas, or rather less than one cubic inch in a minute.

It appears, that less carbonic acid was evolved in this instance in a given time, than when oxygen was respired, but

some circumstances occurred to prevent us from discovering what change the azote had experienced: this point was, however, decided by the following experiment.

Experiment 7. Hydrogen and Oxygen.

Having mixed hydrogen and oxygen gases in such proportion as that the oxygen should rather exceed the quantity contained in atmospheric air, we placed the same animal in the glass A with 66 cubic inches of atmospheric air, 250 cubic inches of the mixture were admitted into gasometer B, from the large water gasometer, and gradually passed through the glass A into gasometer C, during fifteen minutes. The pig did not appear uneasy, and the respired gas measured 250 in C: a portion of this was preserved for examination, which we shall call No. 1.

250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, during thirteen minutes; it measured 250 in C; and a portion No. 2 was preserved for examination.

The animal did not seem to suffer any inconvenience. 250 cubic inches more of the mixture were admitted into B, and gradually passed, as before, through A into C during seventeen minutes. The animal now become quite sleepy, but did not appear to suffer any thing. He was taken out at the end of forty-minutes.

At the close of the experiment, the remains of the mixture, which had stood about an hour in the large water gasometer, being examined was found to contain 22 per cent. of oxygen and no carbonic acid; of the residual 78 parts, 20 were mixed with 10 of oxygen, which had been previously found to contain

3 per cent. azote; these 30 parts being detonated in DAVY's improved VOLTA's eudiometer, by the electric spark, were reduced to 3 parts, and these 3 parts being treated with the tests for oxygen, were reduced to 2 parts, *a proof that all the hydrogen had been consumed*; but the 10 parts of oxygen contained, 3 of azote; this deducted from 2, leaves 1,7 for the azote contained in 20 parts of the residuum 78.

20 : 1,7 :: 78 : 6,6

The mixture employed, therefore, contained in every 100 parts

25	oxygen
6,6	azote
71,4	hydrogen
100	

We next examined the gas which had been respired,

No. 1. 250 cubic inches respired during fifteen minutes.

100 parts, subjected to the action of lime water in PEPYS's eudiometer, were reduced to 93,5, and this by the tests for oxygen was farther diminished to 77: 20 parts of this 77, mixed with 10 of oxygen and detonated, the residuum treated with the tests for oxygen, left 12 parts which were azote,

From these 12 parts

Deduct ,3 for the azote in the 10 parts oxygen

Leaves 11,7 for the azote contained in 20 parts of the residual 77.

20 : 11,7 :: 77 : 45

No. 1, therefore consisted in 100 parts of

6,5	carbonic acid
16,5	oxygen
45	azote
32	hydrogen
100	

No. 2. 250. Respired during thirteen minutes; 100 parts were reduced by lime water to 92,5, and these by the tests for oxygen to 77. Of these 77 parts, 20 being mixed with 10 of oxygen, and detonated, were diminished to 4, and these 4 being examined for oxygen left 3, which must be azote:

From these	3	
Deduct	,3	for azote in the 10 parts oxygen,
Leaves	2,7	for the azote contained in 20 parts of the residual 77.

20 : 2,7 : : 77 : 10,4

No. 2. therefore consisted in 100 parts, of

7,5	carbonic acid,
15,5	oxygen,
10,4	azote,
66,6	hydrogen,
100	

No. 3. 250. respired during seventeen minutes; examined as above, consisted in 100 parts, of

6	carbonic acid,
17	oxygen,
6,5	azote,
70,5	hydrogen,
100	

the 66 remaining with the animal at the close of the experiment, may be considered as very nearly the same as No. 3.

In this, as in the former experiment, we observed that the evolution of carbonic acid was greatest at the middle of the time, but was considerably diminished toward the end, as the pig became sleepy; it is not improbable therefore, that dur-

ing sleep, less carbonic acid is evolved than when the animal is exercising all its faculties.

When atmospheric air alone is respired, we have uniformly found, that the carbonic acid evolved, added to the oxygen remaining, exactly equalled the oxygen existing in the air before it was respired, but in the present instance it was one per cent. more, a circumstance which we are at present unable to account for, but it was constantly the case in all the three trials.

Calculation for Azote.

From the foregoing statement we are enabled to ascertain the quantities of azote, both before and after the experiment.

Azote before the Experiment.

66 cub. inches atmospheric air, with the animal contained $\frac{77}{100}$ or	- - - -	52,14
750 ——— of the mixed gasses contained $\frac{6.6}{100}$ or		49,50
816 total gas employed		101,64

The total azote before the experiment was therefore 101,64 cubic inches.

Azote after the Experiment.

		Respired during.				
No. 1.	250.	15 min.	100 : 45	: : 250 :	112,50
2.	250.	13 min.	100 : 10,4	: : 250 :	26
3.	250.	17 min.	100 : 6,5	: : 250 :	16,25
	66.		100 : 6,5	: : 66 :	4,29
816		45 min.	Azote after experiment		159,04
			Ditto before		101,64
			Increase of azote		57,40

Calculation for Hydrogen.

Hydrogen before Experiment.

The mixture before the experiment was found to contain 71,4 hydrogen.

100 : 71,4 : : 750 : 535,50

therefore the total quantity must be 535,50 cubic inches.

Hydrogen after Experiment.

No. 1.	250	100 : 32	: :	250 :	80
2.	250	100 : 66,6	: :	250 :	166,50
3.	250	100 : 70,5	: :	250 :	176,25
66 in A		100 : 70,5	: :	66 :	46,53
Hydrogen found after experiment					469,28
Hydrogen before the experiment					535,50
Ditto after - - -					469,28
Loss of hydrogen, - -					66,22

In this experiment, as well as in those with oxygen, the proportion of azote evolved, was greater in the early than in the later periods, and it becomes interesting to contrast them: thus we know that 52,14 cubic inches of azote were in the vessel with the animal at the beginning of the experiment, and that, of the 250 cubic inches of mixed gases passed in the first fifteen minutes, only 184 could be expelled into gasometer C, (100 : 6,6 : : 184 : 12,14.) which contained only 12,14

making together 64,28 of azote, which was all that

could have been expected in the first gasometer of 250 after respiration, supposing the *whole* of the common air had been expelled, but we detected 112,50, or an increase of 48,22 cubic inches in fifteen minutes.

The second gasometer before it was connected with the glass A, contained but 16,50 cubic inches of azote; we found however about 26, and what is remarkable, in the last gasometer there was no increase at all.

Calculation for Carbonic Acid.

No. 1.	250.	15 min.	100	: 6,5	: : 250	:	16,25
2.	250.	18 min.	100	: 7,5	: : 250	:	18,75
3.	250.	17 min.	100	: 6	: : 250	:	15
	66.	-	100	: 6	: . 66	:	3,96
		45					53,96

The quantity of carbonic acid evolved in 45 minutes was therefore 53,96 cubic inches, or at the rate of 1,19 cubic inches per minute.

The foregoing experiments seem to prove,

1. That when atmospheric air alone is respired, even by an animal subsisting wholly upon vegetables, no other change takes place in it, than the substitution of a certain portion of carbonic acid gas, for an equal volume of oxygen.

2. That when nearly pure oxygen gas is respired, a portion of it is missing at the end of the experiment, and its place supplied by a corresponding quantity of azote; the portion evolved in a given time, being greater in the early than in the later periods.

3. That the same thing takes place when an animal is made to breathe a mixture of hydrogen and oxygen, in which

the former is in nearly the same proportion to the latter, as azote to oxygen in atmospheric air.

4. That an animal is capable of breathing a mixture of 78 parts hydrogen, and 22 oxygen for more than an hour, without suffering any apparent inconvenience.

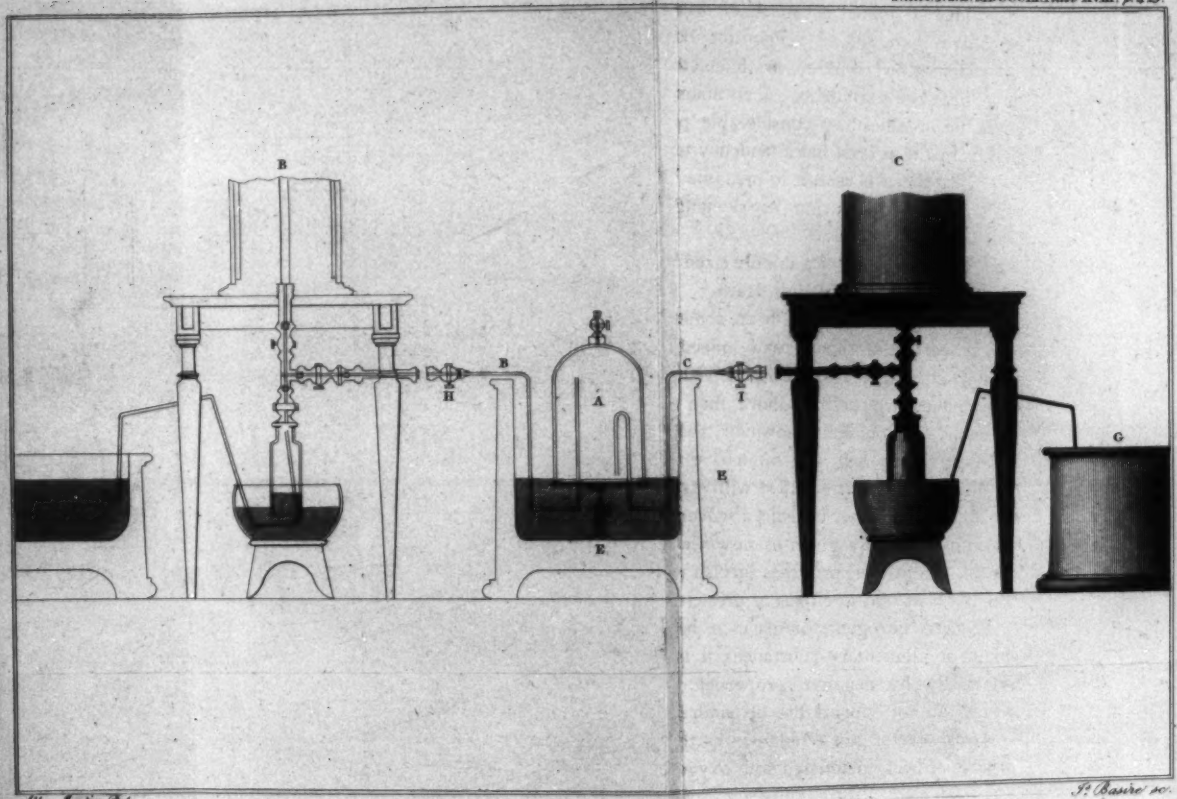
5. That the excitability of an animal is much diminished when he breathes any considerable proportion of hydrogen gas, or that it at least has a tendency to produce sleep.

6. That there is reason to presume an animal evolves less carbonic acid gas during its sleeping, than in its waking hours.

7. That the lungs of a middle sized man contain more than 100 cubic inches of air after death.

These experiments have been conducted without reference to any particular theory, and indeed some of the results were so contrary to our preconceived opinions, that we have been induced to bestow more than ordinary attention on the subject. Confident, however, that all those who repeat the experiments with the same care will arrive at the same results, we shall rest satisfied with stating the facts, not without a hope that those brilliant discoveries of Professor DAVY, which have already given us new views of the operations of nature, will in their progress furnish us with that explanation which it is in vain to expect at present.

Azote or nitrogen, for instance, has been considered as a simple or elementary substance; it is recognised, however, principally by negative properties. Every gaseous fluid which will not support life or combustion, which is not absorbed by water, nor acted upon by the tests for oxygen, nor capable of being detonated with oxygen gas, is generally pro-



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nounced to be azote: it is the constant residuum in almost all our experiments upon gases, but who shall say whether this residuum is a simple substance or a compound?

The experiment of Professor BERZELIUS, leads us to suspect it of metallic properties; and those of DAVY make it probable that it is an oxydated body; the subject is still under discussion. But we may fairly indulge more than a hope that the ardent zeal, and well directed labours of the philosophers just mentioned, will throw a new and important light upon this obscure and difficult subject.

XXVI. *Experiments on Ammonia, and an Account of a new Method of Analyzing it, by Combustion with Oxygen and other Gases; in a Letter to* Humphry Davy, *Esq. Sec. R. S. &c. from* William Henry, *M. D., F. R. S. Y. P. of the Lit. and Phil. Society, and Physician to the Infirmary, at Manchester.*

My Dear Sir,

I should sooner have communicated the account, which you are so good as to request, of my further experiments on the decomposition of ammonia, if I had not been anxious to obtain, by frequent and careful repetition of them, results not affected by any of those numerous causes of error, which easily insinuate themselves into processes of so much delicacy. You have already been informed that the fact, which I lately mentioned to you, (tending to prove the existence of oxygen as an element of the volatile alkali, by the discovery of oxygen gas in the products of its analysis) is not entitled to confidence, owing to the admission of a small quantity of atmospherical air, in a way which was not at all suspected. Frequent repetitions of the same process, under circumstances wholly unobjectionable, have fully satisfied me, that no portion whatsoever of oxygen gas is evolved by electricity from ammonia, even when, by means of an apparatus constructed for the

* This letter in its original form, was read to the Society, May 18th, 1809, some new observations were added and some corrections furnished by the author, in consequence of subsequent experiments made in June; it was transmitted to the Secretary for publication July 10.

purpose, the only metallic surface, exposed to the gas, consists of the sections of two platina wires, each $\frac{1}{10}$ of an inch in diameter, the wires themselves being inclosed in glass tubes which are sealed hermetically round them, and then ground away, so as to expose only the points. Nor does any difference in the nature of the products arise from electrifying the gas either under increased or diminished pressure, the latter of which, it appeared to me probable, from the known influence of elasticity in impeding the combination of gaseous bases, might prevent the oxygen of the alkali from uniting with hydrogen to form water, and occasion the expansion of both into the state of gas.

Having failed, therefore, to acquire, in this way, proof of the existence of oxygen in the volatile alkali, I was next led to look for some unequivocal mode of evincing the production of water by the same operation; a fact, which would be scarcely less satisfactory in establishing oxygen to be one of its constituents, than the actual separation of oxygen gas. The most careful observation of ammonia, during and after the agency of electricity, does not discover the smallest perceptible quantity of moisture. In order, therefore, to subject the gas to a satisfactory test, I had recourse to the following contrivance. Ammoniacal gas, I had previously found, may be so far desiccated by exposure to caustic potash, as to shew no traces of condensed moisture, on the inner surface of a thin glass vessel containing it, when exposed to a cold of 0° Fahrenheit; though the recent gas, by the same treatment, is made to deposit water in the state of a thin film of ice. A glass globe, of the capacity of between two and three cubical inches, was filled with gaseous ammonia, which was then

dried by sticks of pure potash, fastened to pieces of steel wire, so that they could be withdrawn, after having exerted their full action. This point of dryness was ascertained by applying æther, or a mixture of snow and salt, to the outside of the globe. By means of a peculiar apparatus, the gas was next strongly electrified, and the cooling power was again applied to the outer surface of the globe.

In the first trials, that were made with this apparatus, water certainly seemed to have been formed by the electrization of the alkaline gas; for the same portion of gas, which was not affected by a freezing mixture before the process, gave evident signs of condensed moisture, when the cooling power was applied after long continued electrization. The appearance was not only quite satisfactory to myself, but to Mr. Dalton, and several other chemical friends, to whom I shewed the experiment. Finding, however, that the appearance varied as to its degree, I was induced to repeat the process with redoubled precaution; filling the globe, previously heated, with hot mercury, and drying not only the quicksilver, but the iron cistern which contained it, by exposure to long continued heat. The electrified gas now betrayed no signs of moisture on the application of a temperature 20° of Fahrenheit; and gave only the smallest perceptible traces, by a cold of 0° or a few degrees below. I cannot help suspecting, therefore, that the moisture, manifested in the earlier experiments, was derived from the mercury or from some extraneous source, and was not generated by the action of electricity.*

* It may be objected, I am aware, that as the gases produced from ammonia are nearly double its original bulk, they may hold in combination any water that may

The avidity with which ammonia retains moisture, and again absorbs it when artificially dried, is very remarkable. A confined quantity of common air may be completely desiccated, in the space of a few minutes, by pure potash, or by muriate of lime; so that no ice shall appear in the inner surface of the containing vessel, when exposed to a cold of — 26° of Fahrenheit. But ammonia requires exposure during some hours to potash, to stand the test even of 0° Fahrenheit; and a single transfer of the dried gas, through the mercury of a trough in ordinary use, again communicates moisture to it. Muriatic acid gas, freed merely from visible moisture, deposits no water at the temperature of 26° Fahrenheit. This is probably owing to its strong affinity for water; for electricity, after the full action of muriate of lime, evolves, as I have lately ascertained, about $\frac{1}{3}\frac{1}{1}$th its bulk of hydrogen gas, the recent muriatic acid gas giving about $\frac{1}{72}$th after the same treatment.*

have been generated by electricity. But though this supposition may explain the non-appearance of *visible moisture*, it does not account for the inefficiency of a powerful cooling cause to discover traces of watery vapour: for this is a test which renders apparent very minute quantities of water in gases.

* In a course of experiments, which I have described in the Philosophical Transactions for 1800, it appeared that muriatic acid gas, after being dried by muriate of lime, gave nearly as much hydrogen by electrization, as gas which had not been thus exposed. I was not however aware, at that time, of the extreme caution necessary in experiments of this kind; and was satisfied with transferring the acid gas from a large vessel, in which it had been dried, into the electrizing tube, a mode of proceeding which I now find to be quite inadmissible. The action of muriate of lime, which has undergone fusion, on muriatic acid gas, is rendered very sensible, when considerable quantities are used, by the evolution of much heat, and by a diminution of the volume of the gas. Ammonia, also, is contracted in bulk by dry caustic potash. Muriate of lime cannot be employed for its desiccation, since this substance rapidly absorbs

From the average of a great number of experiments on the decomposition of ammonia by electricity, I was for some time led to believe, that you had rather under-stated the proportion of permanent gases obtainable from it by this process, (viz. 108 measures of permanent gas, from 60 of ammonia or 180 from 100). For the most part, I had found the bulk of ammonia to be doubled by decomposition, even when the gas was previously dried with extreme care. In one instance, a small bit of dry potash was left in the tube, along with the ammonia, during electrization, with the view of its absorbing water, which I supposed, at that time, to be generated by the process. In this case, 59 measures (each as 10 grains of mercury) became 115. The following table shews the expansion of various quantities of ammonia.

Exp.		
1.	60 measures of ammonia, gave permanent gas	112
2.	60 - - - - -	120
3.	59 (potash being left in the tube) - -	115
4.	55 - - - - -	115
5.	75 (under the pressure of half an atmosphere)	150
6.	65 - - - - -	130
7.	65 - - - - -	130
8.	53 (one of the conductors being of steel wire)	106
	492	978

and 492 : 978 : : 100 : 198,78. These proportions, you will find, correspond very nearly with those long ago stated

the alkaline gas, even when the gas has been previously exposed to quick-lime. In this case, the ammonia attracts a portion of muriatic acid from the earthy salt, agreeably to the law of affinity, which has been so ably illustrated by Berthollet.

by Berthollet,* who converted 17 measures of ammonia by electrization, into 33 measures of permanent gas, which is at the rate of 194 from 100. Having lately, however, carried on the process with the observance of additional precaution, (the mercury being first boiled in the tube, before admitting the ammonia, and still remaining hot when the gas was passed up), I have obtained from the alkali less than double its volume of permanent gas, viz. 280 measures from 155, or at the rate of 180,6 from 100. The variableness of the first set of results arises, I believe, from the uncertainty of the quantity of ammonia decomposed. For if the smallest portion of moisture remain in the tube, a little ammoniacal gas will be absorbed, and will be slowly given out again as the electrization goes on, thus rendering the actual quantity submitted to experiment greater than appears. It is probable, also, from a fact which I shall afterwards state, that mercury itself, unless when heated, may absorb a small portion of alkaline gas.

The proportion of the hydrogen and nitrogen gases to each other in the products of ammonia decomposed by electricity, I am satisfied, by recent experiments (June, 1809) is as nearly as possible what you have determined, viz. 74 measures of hydrogen gas to 26 of nitrogen. The nearest approximation I have made to these numbers is 73,75 to 26,25. Our only method of analyzing mixtures of these two gases, (viz. by combustion with a redundancy of oxygen) is not, I believe, sufficiently perfect to afford a nearer coincidence.

The extreme labour and tediousness of the decomposition of ammonia by electricity, influenced me to attempt the discovery of a shorter and more summary method of analysis. The

* Journal de Physique, 1786, T. 27.

most obvious one, was its decomposition by oxymuriatic acid gas; but this plan was abandoned, from the impossibility of confining both the gases by any one fluid; since water acts powerfully on the one, and mercury on the other. But a mixture of oxygen and ammoniacal gases more than answered my expectations. When mingled in proper proportions, these gases, I have ascertained, may be detonated over mercury by an electric spark; exactly like a mixture of vital and inflammable air; and the results of the process, with due attention to the circumstances, which will soon be stated, afford an easy and precise method of analyzing, in the space of a few minutes, considerable quantities of the volatile alkali. With a greater proportion of pure oxygen gas* to ammonia than that of three to one, or of ammonia to oxygen than that of three to 1.4, the mixture ceases to be combustible. When the proportions best adapted to inflammation are used, oxygen gas may be diluted with six times its bulk of atmospherical air, without losing its property of burning ammonia.

Atmospherical air alone does not, however, inflame with ammonia, in any proportion that I have yet tried; though, by long continued electrization with air, ammonia is at length decomposed; its hydrogen uniting with the oxygen of the air and forming water, while the nitrogen of both composes a permanent residuum. Forty-five measures of ammonia being electrified with eighty-six of common air, the total 131 became 136, and 132 after being washed with water. Of 17.2 measures of oxygen, contained in the 86 measures of air at the outset, only 2.9 were left, and these, also, would probably have disappeared by continuing the operation. If a mix-

* Containing only three or four per cent. nitrogen gas.

ture of ammonia and atmospheric air, each previously dried by caustic potash and then electrified, be examined, the production of water is made sufficiently apparent on applying ether to the containing vessel. In subjecting ammonia, therefore, to this test of the generation of water by electricity, the purity of the gas from atmospheric air should be carefully determined.*

The products of the combustion of ammonia with oxygen vary essentially, according to the proportion of the gases which are employed. If the oxygen gas exceed considerably the ammonia (that is, if its volume be double or upwards) the ammonia entirely disappears; and no gases remain, but a mixture of nitrogen with the redundant oxygen. The moment the detonation is completed, a dense cloud appears,† and soon afterwards settles into a white incrustation on the inner surface of the tube. The quantity of this substance, which is produced, is too minute for analysis; but its characters resemble those of nitrate of ammonia, the acid ingredient of which is probably generated by the action of oxygen on the nitrogen of one part of the volatile alkali. Accordingly, when the excess of oxygen is removed by sulphuret of lime,

* The result of this experiment shews, moreover, that even supposing oxygen to be a constituent of ammonia, we are not to expect its evolution, in a separate form, by electricity; since, when electrified with ammoniacal gas, oxygen gas is deprived of its elastic form, and its base is condensed into water, by union with nascent hydrogen evolved from the alkali.

† In some cases I have observed, that when the cloud does not occur immediately, it may be made to appear by agitating the quicksilver contained in the detonating tube. This is probably owing to the disengagement of some ammonia, which had lodged in the mercury. The fact confirms what I have already suggested, respecting the cause of the variable proportion of gases, evolved from ammonia by electricity.

the nitrogen generally falls short of the proportion, which ought to accrue from a given weight of ammonia; and hence it is scarcely possible to attain, when a considerable excess of oxygen is used, an accurate analysis of the volatile alkali.

When, on the contrary, the ammonia exceeds considerably the oxygen gas, no production of nitrous acid appears to take place; for the residue, after detonation, is quite free from cloudiness. It is remarkable, however, that ammonia when fired, in certain proportions, with less oxygen than is required to saturate its combustible ingredient, is nevertheless completely decomposed. Part of its hydrogen is sufficient for the saturation of the oxygen; and the remaining hydrogen, and the whole nitrogen of the ammonia, together with that existing as an impurity in the oxygen employed, remain in a gaseous state, and compose a mixture, which may be inflamed by adding a second quantity of oxygen gas, and passing an electric spark.* In this way all the hydrogen of the volatile alkali may be saturated with oxygen, and condensed into water; and the whole of the nitrogen may be obtained as a final result of the process. After determining the amount of the oxygen, consumed both in the first and second combustions, it is easy to calculate the quantity of hydrogen, in the saturation of which it has been employed; for when no nitrous acid has been formed, the hydrogen will be, pretty exactly, double in volume the oxygene which has been expended.

* This is analogous to what happens, when ether, alcohol, or any of the aëriform compounds of carbon and hydrogen, are exploded with a deficient proportion of oxygen: for much of the hydrogen is found in the residuum in the state of gas, and again becomes susceptible of combustion after the addition of a second quantity of oxygen. (See Mr. Cruickshank's excellent papers in the 5th Volume of Nicholson's Journal, 4to.)

These general observations will tend to render the following experiments more intelligible. They may be divided into two classes, 1st, those in which ammonia was fired with an excessive proportion of oxygen; and 2dly, those in which the oxygen, used in the first combustion, was insufficient, or barely adequate, to saturate the whole hydrogen of the alkali.

I. *Decomposition of Ammonia by an Excess of oxygen Gas.*

Twenty-two measures and a third of ammonia were mixed with $44\frac{2}{3}$ oxygen containing 43 of pure gas. The total 67 became 34 when exploded. Water did not produce any farther diminution, but sulphuret of lime left only 8 measures. Now, $34 - 8 = 26$ shews the quantity of oxygen gas, which escaped condensation; and this, deducted from the original quantity (43) gives 17 measures for the amount of the oxygen expended. The last number 17, being multiplied by 2, gives 34 for the hydrogen apparently consumed. The final residue $8 - 1.66$ (the nitrogen introduced by the oxygen gas) $= 6.34$ is the nitrogen obtained from $22\frac{1}{3}$ of ammonia; and if to this the hydrogen be added, 40.34 measures of permanent gas will be the total result. Hence 100 measures of the gas producible from ammonia, should contain 84.29 hydrogen and 15.71 nitrogen; numbers too remote from those, which have been already assigned, to be considered even as approximations to the truth. The error arises from the combination of oxygen, during combustion, not only with the hydrogen, but with the nitrogen of the alkali, the latter of which consequently appears deficient, and the former proportionably in excess.

Frequent repetitions of this combustion, with a considerable excess of oxygen gas, continued to give a deficient proportion of nitrogen; and as no accurate conclusions can be drawn from experiments of this kind, I shall proceed to those of the second class.

II. *Experiments, in which Ammonia was fired with a deficient Proportion of oxygen Gas.*

Sixty-three measures of ammonia were exploded over mercury with 33 of oxygen gas containing one of nitrogen. The total 96, when fired by an electric spark, were diminished to 57 measures, which were not contracted any farther by successive agitation with water, and with sulphuret of lime. The whole of the ammonia, therefore, was decomposed; and all the oxygen had entered into combination with the hydrogen of the alkali. The residuary 57 measures were mingled with 40 measures of the same oxygen gas, and detonated by an electric spark; after which the total, 97, were reduced to 60. The diminution, therefore, was 37 measures; and as two thirds of this number may be ascribed to the condensation of hydrogen gas, the residuary 57 must have been composed of 24.66 hydrogen, and 32.34 nitrogen. The oxygen expended, also, was 32 in the first combustion, + 12.33 in the second = 44.33; and this number, being doubled, gives 88.66 for the whole hydrogen saturated, supposing it to be in the state of hydrogen gas. But from the above quantity of nitrogene (32.34 measures) we are to deduct one measure, with which the 33 measures of oxygen were contaminated; and the remainder 31.34 shews the number of measures of nitrogen, resulting from 63 measures of ammonia. The total amount of gases

obtained is 31.34 + 88.66 = 120; and the proportion of the hydrogen by volume to that of the nitrogen, as 73.88 to 26.12.

To avoid the tediousness of similar details, I shall state, in the form of a table, the results of a few experiments out of a number of others, all of which had, as nearly as could be expected, the same tendency. The sixth experiment in the table is the one which has been just described.

No. of Exp.	Meas. of Ammon. decomposed.	Meas. of Oxygen saturated.	Meas. of Hydrogen estimated.	Meas. of Nitrogen obtained.	Hence 100 Meas. of Ammonia.		Permanent Gas contains in 100 Measures.	
					Take Oxygen.	Give Gas.	Hydr.	Nitr.
1	72	47.5	95	37	66	183	72	28
2	95	64	128	46	67.5	183.8	73.5	26.5
3	100	72.2	144.4	54	72.2	198.5	72.8	28.2
4	74	51.7	103.4	37	69.8	189	73.6	27.4
5	49	33.7	67.4	25.3	68.7	180.2	72.7	27.3
6	63	44.8	88.6	31.3	70.3	193.5	73.9	26.1

From an attentive examination of the foregoing table, it will appear that the results are not perfectly uniform, though perhaps as much as can be expected from the nature of the experiments. Thus the proportion of permanent gases to the ammonia decomposed (the nitrogen being actually measured, and the hydrogen estimated by doubling the oxygen expended) may be observed to differ considerably; the highest product being 198½, and the lowest 180.2, from 100 of ammonia.

There can scarcely be a doubt, however, that this want of coincidence is owing to the same cause, as that which I have already assigned for the variable proportions of permanent gas, which are obtained from equal quantities of ammonia by electrization. And, accordingly, I have found that the evolved gases, as ascertained by combustion, bear the smallest proportion to the ammonia, when most pains have been taken to obviate the presence of moisture. The lowest number, therefore, is to be assumed as most correct; but other circumstances being considered, I believe the second experiment furnishes the most accurate data for determining the composition of ammonia. The same explanation will apply to the different proportions of oxygen gas required for the saturation of 100 measures of ammonia, the variation no doubt arising from the uncertainty of the quantity of alkaline gas which is actually burned. The proportion of oxygen to ammonia, which I believe to be nearest the truth, and most precisely necessary for mutual saturation, is that resulting from the second experiment, *viz.* 67½ measures of oxygen gas to 100 of ammonia, or 100 of the former to 148 of the latter.

It may be observed, also, by comparing the numbers in the two last columns of the table, that the hydrogen and nitrogen gases do not uniformly bear the same proportions to each other. Notwithstanding all the labour I have bestowed on the subject, I have not been able to obtain a nearer correspondence, owing most probably to the imperfection of the mode of analyzing a mixture of hydrogen and nitrogen gases. In the mixture of permanent gases, determined in this way, the hydrogen, it may be remarked, bears generally rather a less ratio than that of 74 to 26. I do not, however, consider this

fact as contradicting the accuracy of the proportions which you have assigned; and it appears to me that a sufficient reason may be given for the want of a more perfect coincidence between results, obtained by such different methods of investigation. In the products of the electrization of ammonia, the hydrogen composes nearly three-fourths of the mixture; and hence its combustion by oxygen gas is likely to be completely effected, and the whole of the hydrogen condensed into water. But after the *partial* combustion of ammonia, by oxygen gas, a residuum is left of hydrogen and nitrogen gases, of which the hydrogen usually composes less, and sometimes considerably less, than one half the bulk. In this case, it may be suspected that a small quantity of hydrogen occasionally escapes being burned; and whenever this happens, its proportion to the nitrogen will appear to be less than the true one.*

From the inflammability of a mixture of ammonia with oxygen gas, it was natural to expect that this alkali would prove susceptible of *slow* combustion. By means of a peculiar apparatus (on a plan which I have described in the Philosophical Transactions for 1808, part II. but on a smaller scale, and with the substitution of mercury for water), I have found that ammonia, expelled from the orifice of a small steel burner, may be kindled by electricity in a vessel of oxygen gas; and that it is slowly consumed with a pale yellow

* This consideration suggests the propriety of using no more oxygen in the first combustion of ammonia, than is barely sufficient to inflame it; or if a larger quantity has been used than is required for this purpose, and a residue consequently obtained, of which the hydrogen forms only a small proportion, it is proper to add a farther quantity of hydrogen, before the second combustion. An allowance may, afterwards be made for this addition.

flame. The combustion, however, is not sufficiently vivid to render the process of any use in the analysis of ammonia.

With nitrous oxide (containing only 5 per cent. impurity) ammonia forms a mixture which is extremely combustible. If the nitrous oxide be in excess, the proportions have a considerable range; for any mixture may be fired by electricity, of which the ammonia is not less than one-sixth of the whole. The combustion is followed by a dense cloud, sometimes of an orange colour. When the nitrous oxide greatly exceeds the ammonia, (as in the proportion, for example, of 100 to 30) there is little or no diminution after firing: and the residuum is composed of a small portion of undecomposed oxide, some oxygen gas, and a considerable quantity of nitrogen, the last of which, however, is not in its full proportion. When the nitrous oxide is further increased, still more oxygen is found in the residuum.

When, on the contrary, the alkaline gas is redundant, combustion does not take place unless the nitrous oxide forms one-third of the mixture. A little diminution takes place on firing, but no cloudiness is observed; and the residue is composed of hydrogen and nitrogen gases, with occasionally a small portion of undecomposed ammonia. As an example of what takes place, I select the following experiment from several others.

A mixture of 41 measures of ammonia, with 40 of nitrous oxide(= 38 pure), in all 81 measures, were reduced by combustion to 75, which were found to consist of 16 hydrogen and 59 nitrogen gases. To explain this experiment, we may assume (as is consistent with your own analysis*) that 100

* Researches, Res. ii. Div. 1, or Thomson's System of Chemistry, 3d edit. ii. 143.

measures of nitrous oxide, are equivalent to 52 measures of oxygen gas and 103 of nitrogen. The oxygen in 38 measures of nitrous oxide will, therefore, be 19,7, to which, when the oxygen spent in burning the residuum (viz. 8 m. is added, we obtain 27,7 for the total oxygen consumed; and multiplying by 2, we have 55,4 for the hydrogen saturated. From the residuary nitrogen (59) deduct 39 measures arising from the decomposition of the nitrous oxide + 2 m., mingled with it as an impurity = 41, and the remainder, 18 measures, is the nitrogen resulting from the volatile alkali; and as 41 measures of ammonia give 55,4 + 18 = 73,4 measures of permanent gas, 100 would give 179 measures, in which the hydrogen and nitrogen would exist in the proportion of 75,4 to 24,6. From the same facts it may be deduced, that 100 measures of ammonia require for saturation 130 of nitrous oxide = 67½ oxygen gas. The coincidence then, between the results of the combustion of ammonia with nitrous oxide, and those with oxygen gas, confirms the accuracy of both methods of analysis.

NITROUS GAS, which, it appears from your testimony,* does not compose an inflammable mixture with hydrogen, (nor as I am assured by Mr. DALTON, with any of the varieties of carburetted hydrogen) may be employed, I find, for the combustion of ammonia. The proportions required for mutual saturation are about 120 measures of nitrous gas to 100 of ammonia. An excess of the former gas does not give accurate results; since not only the hydrogen of the ammonia, but some of its nitrogen is also condensed; and the mixture, after being fired, exhibits the cloudy appearance usual in that case.

* Researches, p. 136.

Forty-eight measures of ammonia, being fired with 60 nitrous gas, (= 53 pure) both gases were completely decomposed; and a residue left consisting of 61 nitrogen and 9 hydrogen. Sixty measures of ammonia and 41 nitrous gas (= 36,1 pure) gave, after firing, a mixture composed of 10 ammonia, 53½ nitrogen, and 50½ hydrogen. But taking for granted that 100 measures of nitrous gas, according to your analysis, hold in combination a quantity of oxygen equal to 57½ measures of oxygen gas, and of nitrogen equal to 48½ measures, and assuming the proportions of the nitrogen and hydrogen in ammonia, to be those established by your experiments and my own. It will appear from an easy calculation, that the proportion of nitrogen, in the above residua, a little exceeds, and that of the hydrogen rather falls short of what might have been expected. I have not yet been able to reconcile these differences, by the numerous trials required in a process of so much delicacy; and I reserve the enquiry for a season of more leisure. The foregoing statement, I wish to be considered as merely announcing the general fact of the combustibility of a mixture of ammonia and nitrous gas, a property which chiefly derives importance, from its being capable of application to a new method of analysing the latter.

Before concluding this letter, I shall briefly state the results of some experiments, which I have lately made in conjunction with Mr. Dalton, on a subject that formerly occupied much of my attention; viz. the effect of electricity on the aëriform compounds of carbon and hydrogen. Subsequent

reflection, as well as the candid and judicious criticisms of various writers,* have influenced me to doubt of the accuracy of a few of the conclusions drawn from my former inquiries.† The knowledge of this class of bodies has, also, been so materially advanced during the last twelve years, that the examination of their properties may now be undertaken, with much greater confidence and success than formerly. It is to be lamented, indeed, that experimentalists do not oftener retrace their labours, with the combined advantages of acquired skill, and of a more improved state of the science which they investigate.

The gases, submitted by Mr. Dalton and myself to the action of long continued electrization, were carburetted hydrogen from pit-coal of the specific gravity of about 630 (air being 1000) olefiant gas, and carbonic oxide. Each gas was used in as pure a state as possible; muriate of lime being first introduced into the same tubes in which the gases were electrified, and being withdrawn when it had exerted its full action. Platina wires were used to convey the electric discharges.

When the electrization of carburetted hydrogen or olefiant gases was continued sufficiently long, they were each found to expand, notwithstanding their extreme dryness. No carbonic acid could be discovered in the electrified gas by the nicest tests. When fired with oxygen, it gave less carbonic

* See Berthollet's Chemical Statics, Eng. trans. Vol. II. p. 454; Murray's Elements of Chemistry, Vol. II. Note G; a letter from an anonymous correspondent in Nicholson's Journal, 8vo. II. 185; and Aikin's Dictionary of Chemistry, I. 251.

† "Experiments on Carbonated Hydrogen Gas, with a View to determine whether Carbon be a simple or a compound body." Phil. Trans. Vol. LXXVII.

acid than the unexpanded gas, and required less oxygen for saturation. Calculating, from the diminished product of carbonic acid, how much gas had been decomposed by electrization, it appeared that the decomposed part, in all cases, was about doubled. The smaller product of carbonic acid from the electrified gas, was sufficiently explained by a deposition of charcoal on the inner surface of the glass tube, too distinct to be at all equivocal, and most abundant from the olefiant gas. No addition whatsoever of nitrogen was made by the electrization. It appears, therefore, that the hydro-carburetted gases, like ammonia, are separated by electrization into their elements, the carbon being precipitated, and the hydrogen evolved in a separate form, and acquiring a state of greater expansion. This change, however, is effected much more slowly, than the disunion of the elements of ammonia.

From a portion of carbonic acid gas, carefully dried by muriate of lime, and electrized with platina conductors, we obtained, after removing the undecomposed gas by caustic potash, a residuum equal to about one twentieth the whole gas which had been employed. It was found on analysis to consist of oxygen and carbonic oxide gases, in such proportions as to inflame on passing an electric spark through it without any addition, and to be thus convertible again into carbonic acid. In the experiments of M. SAUSSURE, jun.* that ingenious philosopher obtained only carbonic oxide by the same operation, owing doubtless to the electricity having been conveyed by conductors of copper, which would become oxidized, and prevent the oxygen from being evolved in a separate form.

Carbonic oxide, electrified with similar precautions, did not

* Journal de Physique, Tom. LIV. p. 430.

appear to undergo any change. Eleven hundred discharges from a Leyden jar had no effect on a quantity of the gas, equal to about one tenth of a cubic inch. Its bulk, after this process, was unaltered; no carbonic acid could be discovered in it; and there was no decided trace of oxygen gas in the residuum. The carbon, it appears, therefore, which exists in carbonic oxide, must be held combined by an extremely strong affinity.

With sincere esteem and respect, I am,

Dear Sir,

Your faithful and obliged friend,

WM. HENRY.

XXVII. *New analytical Researches on the Nature of certain Bodies, being an Appendix to the Bakerian Lecture for 1808.* By Humphry Davy, Esq. Sec. R. S. Prof. Chem. R. I.*

1. *Further Inquiries on the Action of Potassium on Ammonia and on the Analysis of Ammonia.*

THE most remarkable circumstances occurring in the action of potassium upon ammonia are the disappearance of a certain quantity of nitrogene, and the conversion of a part of the potassium into potash.

The first query which I advanced in the last Bakerian Lecture, on this obscure and difficult subject, was whether the gas developed in the first part of the process of the absorption of ammonia by potassium is hydrogene, or a new species of inflammable aeriform substance, the basis of nitrogene?

Experiments made to determine this point have proved, as I expected, that the gas differs in no respect from that given out during the solution of zinc in sulphuric acid; or that produced during the action of potassium on water. By slow combustion with oxygene, it generates pure water only, and

* The account of the principal facts respecting the action of potassium on ammonia, in this communication, were read before the Royal Society, February 2, 1809. The paper was ordered to be printed March 16, 1809. At that time, having stated to the Council that I had since made some new experiments on this matter, and on the subjects discussed in the Bakerian Lecture for 1808, I received permission to add them to the detail of the former observations for publication.

its weight, in a case in which it was mixed with atmospherical air, precisely corresponded with that of an equal quantity of hydrogene.

Another query which I put is, has nitrogene a metallic basis which alloys with the metals employed in the experiment?

This query I cannot answer in so distinct a manner; but such results as I have been able to obtain are negative.

I have examined the potassium generated in the process. It has precisely the same properties as potassium produced in the common experiment of the gun-barrel; and gives the same results by combusion in oxygene, and by the action of water.

In cases in which I had distilled the olive-coloured fusible substance in an iron tray, the surface of the tray appeared much corroded, the metal was brittle, and appeared crystallized. I made a solution of it in muriatic acid; but hydrogene alone was evolved.

I distilled a quantity of the fusible substance from 9 grains of potassium in an iron vessel, which communicated with a receiver containing about 100 grains of mercury, and by a narrow glass tube the gas generated was made to pass through the mercury; the object of this process was to detect if any of the same substance, as that existing in the amalgam from ammonia, was formed; but during the whole period of distillation, the mercury remained unaltered in its appearance, and did not effervesce in the slightest degree when thrown into water.

That the nitrogene which disappears in this experiment is absolutely converted into oxygene and hydrogene, and that its elements are capable of being furnished from water, is a

conclusion of such importance, and so unsupported by the general order of chemical facts, that it ought not to be admitted, except upon the most rigid and evident experimental proofs.

I have repeated the experiment of the absorption of ammonia by potassium in trays of platina or iron, and its distillation in tubes of iron more than twenty times, and often in the presence of some of the most distinguished chemists in this country, from whose acuteness of observation, I hoped no source of error could escape.

The results, though not perfectly uniform, have all been of the same kind as those described in page 55. Six grains of potassium, the quantity constantly used, always caused the disappearance of from 10 to 12.5 cubical inches of well dried ammonia. From 5.5 to 6 cubical inches of hydrogene were produced, a quantity always inferior to that evolved by the action of an equal portion of the metal upon water. In the distillation from 11 to 17 cubical inches of elastic fluid were evolved, and from 1.5 to 2.5 grains of potassium regenerated.

The quantity of ammonia in the products, varied from a portion that was scarcely perceptible to one twelfth or one thirteenth of the whole volume of elastic fluid: and it was least in those cases in which the absence of moisture was most perfectly guarded against. Under these circumstances likewise, more potassium was revived; and the unabsorbable elastic fluid, and particularly the hydrogene in smaller proportion.

When the products of distillation were collected at different periods, it was uniformly found that the proportion of nitrogene to the hydrogene diminished as the process advanced.

The first portions contained considerably more nitrogene in proportion, than the gases evolved during the electrization of ammonia, and the last portions less.

I shall give the results of an experiment, in which the gases produced in distillation were collected in four different vessels, and in which every precaution was taken to avoid sources of inaccuracy.

The barometer was at 29.8, thermometer 65° FAHRENHEIT.

6 grains of potassium absorbed 12 cubic inches of well dried ammonia. The metal was heated in a tray of platina, and the gas contained in a retort of plate glass.

5.8 cubical inches of hydrogene were produced.

The fusible substance was distilled in an iron tube of the capacity of 3 cubical inches and half filled with hydrogene, the adaptors connected with the mercurial apparatus contained .8 of common air.

The first portion of gas collected (the heat being very slowly raised, and long before it had rendered the vessel red), equalled 7.5 cubical inches. It contained .6 of ammonia, 7 of the residuum detonated with $4\frac{1}{2}$ of oxygene gas left a residuum of 4.

The second portion, equal to 3 cubical inches, contained no ammonia. 7.2 measures of it, detonated with 3.8 of oxygene, left a residuum of 3.5.

The third portion was equal to 5 cubical inches; at this time the tube was white hot; it contained no ammonia, 8.5 of it detonated with 4.5 of oxygene diminished to 2.5.

The last portion was a cubical inch and half, collected when the heat was most intense. 4.5 measures, with 3.75 of oxygene, left a residuum of 2.8.

The iron tube contained, after the experiment, (as was ascertained by admitting hydrogene when it was cool), 9.7 of gas; which seemed of the same composition as the last portion. The adaptors must have contained .8 of a similar gas.

The tube contained potash in its lowest part, and in its upper part potassium, which gave by its action upon water 1¼ cubical inch of hydrogene.

As the largest quantity of hydrogene is always produced at that period of the process, in which the potassium must be conceived to be regenerated, and in which the gases being in the nascent state, its power of action upon them would be greatest, it occurred to me, that if nitrogene was decomposed in the operation, there would probably be a larger quantity of it destroyed by the distillation of the fusible substance, with a fresh quantity of potassium, than by the distillation of it in its common state. On this idea I made several experiments; the results did not differ much from each other, and were such as I had expected. I shall describe one process made with the same apparatus as that which I have just detailed.—Barometer was at 29.5, thermometer 70° FAHRENHEIT.

6 grains of potassium were employed in an iron tray; 10 cubical inches of ammonia were absorbed, a small globule of metal remained unconverted into the fusible substance. A fresh piece of potassium, weighing six grains, was introduced into the tray.

The iron tube and the adaptors (having together a capacity equal to 4.3 cubical inches) contained common air.

The gas was collected in three portions, there was no absorbable quantity of ammonia in either of them.

The first portion, that produced before the tube became red,

was eight cubical inches. 10.25 of it detonated with 8.5 of oxygene diminished to 8.

The second portion equalled five cubical inches; 9½ of this, with 5 of oxygene, left a residuum of 3¼.

Of the third portion, 2 cubical inches and ½ came over. 9 of it, detonated with 5 of oxygene gas, left a residuum of 1.4.

The iron tube and the adaptors contained, at the end of the experiment, as was proved by cooling and the admission of hydrogene 2.3 cubical inches of gas, which appeared of the same composition as the third portion. Nearly 7 grains of potassium were recovered.

A comparison of these results, with those stated in the preceding page, will fully prove, that there is a much smaller proportion of nitrogene to the hydrogene, in the case in which the olive-coloured substance is distilled with potassium, than in the other case, and there is likewise a larger quantity of potassium converted into potash.

The loss of nitrogene, and the addition of oxygene to the potassium, are sufficiently distinct in both processes; and the want of a correspondence between these results, and those of the experiment detailed in page 55, are not greater than might be expected, when all the circumstances of the operation are considered. In the instance, in which a double quantity of potassium was employed, more potash must have been formed from the oxygene of the common air in the tubes; and the fusible substance, in passing through the atmosphere, absorbs in different cases different quantities of oxygene and of moisture; during the intervals of the removal of the different portions of gas likewise, some globules are lost.

In instances when the heat has been more rapidly raised, I

have generally found more potassium destroyed, and less nitrogene in proportion in the aeriform products. In such cases likewise, the loss of weight has been much greater; the gases have been always clouded, and the adaptors, after being exposed to a moist air, emitted a smell of ammonia; from which it seems likely that small quantities of the dark gray substance described in page 50 of this Volume, are sometimes carried over undecompounded in the operation.

In some late experiments, I substituted for the iron tube, a tube of copper, which had been bored from a solid piece, and the sides of which were nearly a quarter of an inch in thickness. My object in using this tube was not only to prevent the heat from being too rapidly communicated to the fusible substance, but likewise to be secure that no metallic oxide was present, for though the iron tubes had been carefully cleaned, yet still it was possible that some oxide, which could not be separated from the welded parts, might exist, which of course would occasion the disappearance of a certain quantity of potassium.

I shall give the results of one of the processes, which I regard as most correct, made in the tube of copper. The barometer was at 30.5; thermometer was at 59° FAHRENHEIT.

The tube contained two cubical inches and half, and was filled with hydrogene.

6 grains of potassium, which had absorbed 13 cubical inches of ammonia in a copper tray were employed.

The adaptors connected with the mercurial apparatus and the stop-cocks, contained .7 of atmospherical air.

The gas given off was collected in two portions.

The first portion was equal to 11 cubical inches. It con-

tained .8 of ammonia, 11 of the residuum, detonated with 8 of oxygene, left 8.

The second portion equalled 2 cubical inches. They contained no ammonia. 10 of this gas, with 8 of oxygene detonated, left a residuum of 10.

There remained in the tube and adaptors 1.1 cubical inch of gas.

The quantity of hydrogene produced by the action of the potassium, which had been regenerated, equalled 4.5 cubical inches.

In this experiment the heat was applied much more slowly than in any of those in which the iron tube was used, and even at the end of the operation, the temperature was little more than that of cherry red.

In the upper part of the stop-cock there was found a minute quantity of gray powder, which gave ammonia by the operation of moisture.

In no case, in which I have used the copper tube in like processes of slow distillation, has there been less than 4 grains of potassium revived; and the proportion of nitrogene to the hydrogene in the gas evolved has been uniformly much greater than in processes of rapid distillation in the tubes of iron; but the whole quantity of elastic matter procured considerably less.

Copper has a much stronger affinity for potassium * than

* Copper heated in potassium speedily dissolves, and diminishes its fusibility; but potassium requires a white heat to enable it to combine with iron. In another experiment, in which I distilled the fusible substance in an iron tray, contained in the copper tube, a considerable quantity of copper, that had been dissolved, was found in the state of powder deposited upon the tray, or loose in the bottom of the tube.

iron. It occurred to me as probable, that this attraction, by preventing the potassium from rising in vapour at its usual temperature, and likewise by the general tendency of such combination to give greater density, might occasion a diminution of its action upon the nitrogene in the nascent state. Ammonia has a strong attraction for the oxide of copper, and it consequently is not unlikely that the fusible substance may combine with metallic copper, and that this compound may not be entirely destroyed in the distillation. And assuming this, it may be conceived that the loss of hydrogene partly depends upon some combination of the basis of ammonia with copper.

I had a tube, of the capacity of $2\frac{1}{4}$ cubical inches, made of wrought platina, cemented by means of fine gold solder. The fusible substance was obtained (as usual from six grains of potassium) in a tray of platina, where it was brought in contact with a large surface of platina wire; the distillation was slowly conducted; but before the temperature of the tube had approached to that of ignition, it dissolved and gave way at the points where it was soldered, and a violent combustion took place. Only 7 cubical inches of gas were collected; but of this, allowing for the hydrogene that filled the tube, nearly $\frac{3}{5}$ were nitrogene.

I am making preparations for performing the experiment in a bored tube made from a single piece of platina, and likewise in tubes made of other metals, and I hope to be able, in a short time, to have the honour of laying the results before the Society.

I shall make no apology for bringing forward the investigation in its present imperfect state, except by stating that my

motive for so doing, is the desire of being assisted or corrected by the opinions and advice of the learned chemical philosophers belonging to this illustrious body. In an investigation connected with almost all the theoretical arrangements of chemistry, and in operations of so much delicacy, it will, I conceive, be allowed, that it is scarcely possible to proceed with too much caution, or to multiply facts to too great an extent.

The different phenomena presented by the processes of distillation in different metallic tubes, may lead to new explanations of this intricate subject, and though the facts cannot be easily accounted for, except on the supposition that nitrogene is an oxide, yet till the proportions and weights are distinctly ascertained, the inquiry cannot be considered as far advanced, for in an experiment, in which the processes are so complicated and delicate, and in which the data are so numerous, it is not easy to be satisfied that every source of error has been avoided, and that every circumstance has been examined and reasoned upon.

All conclusions on the action of potassium on ammonia, are immediately dependent upon the results of the electrical analysis of the volatile alkali. In a letter, which I received in the course of the last month from Dr. Henry, that excellent chemist, has stated that he conceives I have rather underrated the quantity of nitrogene in ammonia, according to the proportions given in the Bakerian Lecture for 1807. This notice has induced me to repeat the experiment, under new circumstances, and I find not the slightest reason for doubting of the entire accuracy of my former results.

In the new trial, I used mercury which had been recently

boiled in the tube for electrization; the ammonia was introduced after being long dried by caustic potash, from a receiver in which it had not been generated, and which had likewise been inverted over boiling mercury. The gas left no perceptible residuum, when absorbed by water deprived of air by boiling. In this process, 15 measures of ammonia expanded, so as to fill 27 measures; and the hydrogene by detonation with oxygene, over water freed as much as possible from air, proved to be to the nitrogene as 73.8 to 26.2. In the experiment three explosions were made, the oxygene being deficient in the first two; so that no nitrogene could have been condensed in the form of nitric acid.

Except when precautions of this kind are employed, as I have before noticed, no accurate data can be obtained respecting the proportions of permanent gases obtained from ammonia by electricity.

When the gas is generated and decomposed over the same mercury, there is always a greater expansion than the true one; and when the mercury is not boiled in the tube, and when common water is used, the nitrogene will be always over-rated, unless this error is counteracted by an opposite error, that of detonating with an excess of oxygene.*

Dr. HENRY had the kindness to send me the apparatus, in which he conceived, at that time, that he had witnessed the formation of water in the decomposition of ammonia by elec-

* It will be seen by Dr. HENRY's letter, which immediately precedes this communication, that in repeating his processes, since this paper was written, he has gained results almost precisely the same as those indicated in the text; and there is every reason to believe, that 100 of ammonia in volume uniformly become 180, when decomposed by electricity, and that the gas produced consists in 100 parts of 74 hydrogene and 26 nitrogene.

tricity, by his ingenious method of applying hygrometrical tests.

I tried one experiment only with it, and in this there seemed to me to be more moisture exhibited in the elastic matter after electrization than before, when it was cooled by the evaporation of ether: but on maturely considering this question, I do not think that the appearance of moisture even offers a decided proof of the existence of loosely combined oxygene in ammonia. To common hygrometrical tests, water must be less sensible in ammonia than in hydrogene or nitrogene, from its tendency to be precipitated in the form of alkaline solution, and likewise probably from its having a stronger adherence to the gas; and the elastic fluid generated, from the increase of volume will be capable of containing more aqueous vapour.

It is not easy to determine, with perfect precision, the specific gravity of a gas, so light as hydrogene and even ammonia; but the loss of weight, which appears to take place in the electrical analysis of ammonia, cannot, I think, with propriety, be referred entirely to this circumstance; whether the solution that I have ventured to give* be the true one, I shall not, in the present state of the inquiry, attempt to discuss.

The question of ammonia being analogous to other salifiable bases in its constitution, is determined by the phænomena presented by the amalgam from that alkali; and if the conversion of nitrogene into oxygene and hydrogene should be established, it would appear that both hydrogene and nitrogene must be different combinations of ammonium with oxygene, or with water.

* Bakerian Lecture, 1807, p. 40.

II. *Further Inquiries respecting Sulphur and Phosphorus.*

I have stated, in the last Bakerian Lecture, that hydrogene is produced from sulphur and phosphorus in such quantities, by VOLTAIC electricity, that it cannot well be considered as an accidental ingredient in these bodies. I have likewise stated, that when potassium is made to act upon them, the sulphurets and phosphurets evolve less hydrogene in the form of compound inflammable gas by the action of an acid, than the same quantity of potassium in an uncombined state, and from this circumstance, I have ventured to infer, that they may contain oxygene.

On the idea, that sulphur and phosphorus are deprived of some of their oxygene by potassium, it would follow, that when the compounds formed in this experiment are decomposed, these substances ought to be found in a new state; deoxygenated, as far as is compatible with their existence in contact with water.

With the view of examining the nature of the substances, separated by the action of muriatic acid upon the sulphurets and phosphurets of potassium, I combined a few grains of sulphur and phosphorus, with one fourth of their weight of potassium, and exposed the compounds to the action of a strong solution of muriatic acid. As in the former cases, less inflammable gas was produced than would have been afforded by equal quantities of the uncombined potassium, and considerable quantities of solid matter separated from both compounds, which after being washed were collected in a filter.

The substance which separated from the sulphuret, was of

a dark gray colour,* and was harsh to the touch; it had no taste, and at common temperatures no smell; but when heated, it emitted the peculiar odour of sulphur. Its specific gravity was rather less than that of sulphur. It softened at a low heat, so as to be moulded like wax between the fingers. It was a non-conductor of electricity. When heated upon a surface of glass, it soon fused, entered into ebullition, took fire, and burnt with the same light blue flame as sulphur. A small particle of it, made to combine with silver, presented the same phenomena as sulphur.

The substance from the phosphuret was of an amber colour, and opaque. It could not be examined in the air, in the form in which it was collected (that of a loose powder) for as soon as it was wiped dry, it took fire, and burnt in the same manner as phosphorus; when melted under naphtha, it was found to differ from phosphorus, in being much deeper coloured, perfectly opaque, and very brittle. Its fusibility was nearly the same, and, like common phosphorus, it was perfectly non-conducting.

In experiments upon the union of potassium with sulphur and phosphorus the heat is so intense, that when larger quantities than a few grains are used, the glass tubes are uniformly fused or broken in pieces, and in consequence I have not been able to operate upon such a scale, as to make an accurate examination of the substances just described, and to determine the quantity of oxygene they absorb in being converted into acid. Metallic vessels of course cannot be employed; but I

* Possibly this colour may have been produced by the decomposition of a film of soap of naphtha adhering to the potassium.

intend to try tubes of porcelain, in a further investigation of the subject.

It is evident that the sulphur and phosphorus, separated in these processes, are not in their common state; and the phenomena would certainly incline one to believe that they ar less oxygenated. It may, I know, be said, that it is possible that they are merely combined with more hydrogene, and that the sulphur in this state is analogous to the hydrogenated sulphur of BERTHOLLET, and to the alcohol of sulphur of LAMPADIUS.

But when I decompounded dry sulphuret of potash by muriatic acid, of the same kind as had been used for decompounding the sulphuret of potassium, the substance produced seemed to be merely in that form, in which, according to the able researches of Dr. THOMSON, it is combined with water; and notwithstanding the ingenious experiments of M. A. BERTHOLLET and M. ROBIQUET,* the nature of the substance produced during the passage of sulphur over ignited charcoal is far from being fully ascertained. In a series of experiments, which my brother, Mr. JOHN DAVY, had the goodness to undertake, at my request, in the laboratory of the Royal Institution, on the action of sulphur on charcoal, the products were found to be very different, according as the charcoal employed differed in its nature. In an instance, in which imperfectly made charcoal was employed, the liquor that passed over left by combustion a residuum that had all the properties of carbonaceous matter, which agrees with the observations of M. M. DESORMES and CLEMENT; but when the charcoal had been well burnt, there was no such residuum produced. It was found, that the same

* *Annales de Chimie*, 1807, page 144, 148.

charcoal might be employed in a number of processes till it was nearly entirely consumed, and that the sulphur, not rendered liquid, might be used for several operations. In all cases mixtures of * sulphuretted hydrogene gas and hydrocarbonate were evolved.

I particularly examined a specimen of the liquor which had been obtained in the last process from charcoal that had been often used. It was a non-conductor of electricity, and when the VOLTAIC spark was taken in it, did not evolve gas with more rapidity than sulphur; and this gas proved to be sulphuretted hydrogene.

Supposing the liquor to contain hydrogene in considerable quantities, I conceived that it must be decomposed by oxymuriatic acid; but it merely absorbed this substance, depositing crystals of common sulphur, and becoming a fluid similar to the sulphuretted muriatic acid; though when water was introduced, hydrated sulphur was instantly formed, and muriatic acid gas evolved.

From the quantity of carbonic acid formed by the combustion of the carburetted inflammable gas, produced in the operation of the action of well burnt charcoal upon sulphur, it may be conceived to contain oxygene. This circumstance, and the fact that no hydrate of sulphur or muriatic acid gas is formed by the operation of oxymuriatic acid upon the liquor, but common sulphur precipitated; are in favour of the opinion, that the sulphur in this liquor, contains less oxygene than in its common state. This idea has likewise occurred to

* Five measures of the mixed gas, agitated with solution of potash, left a residuum of 3.5. These were detonated with 5.5 of oxygene; the whole diminution, was to 6, of this residuum 2.5 appeared to be carbonic acid.

Dr. MARCET, who is engaged in some experiments on the subject, and from whose skill and accuracy, further elucidations of it may be expected.

III. *Further Inquiries respecting carbonaceous Matter.*

On the idea which I have stated, page 73, that the diamond may consist of the carbonaceous matter combined with a little oxygene, I exposed charcoal intensely ignited, by VOLTAIC electricity,* to nitrogene, conceiving it possible that if this body was an oxide, containing oxygene very intimately combined, it might part with it in small proportions to carbonaceous matter, and give an important result.

The charcoal, which had been made with great care, was preserved for a quarter of an hour in a state of ignition, in which platina instantly fused. It did not appear to change in its visible properties; but a small quantity of black sublimate, which proved to be nothing more than finely divided carbonaceous matter, collected in an arborescent state upon the platina wire to which the charcoal was attached. The gas had increased in volume one sixth; but this was owing to the evolution of carburetted inflammable gas from the charcoal, the nitrogene was unchanged in quantity, and far as my examination could go, in quality. The points of the charcoal where the heat had been intense, were rather harder than before the experiment.

I have mentioned, page 102, that charcoal, even when strongly ignited, is incapable of decomposing corrosive sublimate. When charcoal, in a state of ignition, is brought in

* The apparatus was the same as that referred to page 59. The power employed was that of the battery of 500 belonging to the Royal Institution.

contact with oxymuriatic acid gas, the combustion instantly ceases. I electrified two pieces of charcoal in a globe filled with oxymuriatic acid gas, which had been introduced after exhaustion of the globe. They were preserved, for nearly an hour, in intense ignition, by the same means that had been employed in the experiment on nitrogene. At first, white fumes arose, probably principally from the formation of common muriatic acid gas, by the action of the hydrogene of the charcoal upon the oxymuriatic acid, and the combination of the gas so produced, with aqueous vapour in the globe; but this effect soon ceased. At the end of the process, the oxymuriatic acid gas was found unaltered in its properties, and copper leaf burnt in it with a vivid light. The charcoal did not perceptibly differ from the charcoal that had been exposed to nitrogene. My view in making this experiment, was to ascertain whether some new combination of carbonaceous matter with oxygene might not be formed in the process, and I hoped likewise to be able to free charcoal entirely from combined hydrogene, and from alkaline and earthy matter, supposing they existed in it, not fully combined with oxygene. That hydrogene must have separated in the experiment, it is not possible to doubt, and on evaporating the deposit on the sides of the globe, which was in very minute quantity, and acted like concentrated muriatic acid, it left a perceptible saline residuum.*

* Charcoal, over which sulphur has been passed, as in the experiments, page 465, as has been shewn by M. A. Berthollet, contains sulphur, and this I find after being heated to whiteness; such charcoal is a conductor of electricity, and does not differ in its external properties from common charcoal.

IV. *Further Inquiries respecting muriatic Acid.*

The experiments on muriatic acid, which I have already had the honour of laying before the Society, shew that the ideas which had been formerly entertained respecting the difference between the muriatic acid and the oxymuriatic acid are not correct. They prove that muriatic acid gas is a compound of a substance, which as yet has never been procured in an uncombined state, and from one-third to one-fourth of water, and that oxymuriatic acid is composed of the same substance, (free from water) united to oxygene. They likewise prove, that when bodies are oxydated in muriatic acid gas, it is by a decomposition of the water contained in that substance, and when they are oxydated in oxymuriatic acid, it is by combination with the oxygene in that body, and in both cases there is always a union of the peculiar unknown substance, the dry muriatic acid with the oxydated body.

Of all known substances belonging to the class of acids, the dry muriatic acid is that which seems to possess the strongest and most extensive powers of combination. It unites with all acid matters that have been experimented upon, except carbonic acid, and with all oxides (including water), and all inflammable substances that have been tried, except those which appear to be elementary, carbonaceous matter and the metals; and should its basis ever be separated in the pure form, it will probably be one of the most powerful agents in chemistry.

I have lately made several new attempts to procure uncombined dry muriatic acid; but they have been all unsuccessful.

I heated intensely, in an iron tube, silex in a very minute state of division, and muriate of soda that had been fused; but there was not the smallest quantity of gas evolved. In this case, the silex had been ignited to whiteness before it was used; but when silex in its common state was employed, or when aqueous vapour was passed over a mixture of dry silex and dry salt in a porcelain tube, muriatic acid gas was developed with great rapidity.

I have stated, page 79, that a sublimate is formed by the combustion of the olive-coloured oxide of boracium in oxymuriatic acid. On the idea that this might be boracic acid, and that dry muriatic acid might be separated in the process, I examined the circumstances of the experiment; but I found the sublimate to be a compound of boracic and muriatic acid, similar to the compound of muriatic and phosphoric acid.

I heated freshly sublimed muriate of ammonia with potassium; when the quantities were equal, as much hydrogene gas was developed as is generated by the action of water on potassium; much ammonia was evolved, and muriate of potash formed; when the potassium was to the muriate as 4 to 1, less hydrogen appeared, and a triple compound of muriatic acid, ammonia, and potassium, or its protoxide was formed, which was of a dark gray colour, and gave ammonia and muriate of potash by the action of water. There was not the slightest indications of the decomposition of the acid in the experiment. The process, in which this decomposition may be most reasonably conceived to take place, is in the combustion of potassium in the phosphuretted muriatic acid, deprived

by simple distillation with potassium of as much phosphorus as possible. I am preparing an apparatus for performing this experiment, in a manner which, I hope, will lead to distinct conclusions.

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INDEX

TO THE

PHILOSOPHICAL TRANSACTIONS,

FOR THE YEAR 1809.

A

page

Air, atmospheric, experiments on its respiration, - 413
Albumen, observations on, and some other animal fluids, with remarks on their analysis, - - - - - 378
——— observations on some animal fluids containing it, 380
Alburnum of trees, its functions, - - - 173
Allen, William, Esq. His experiments, made in conjunction with William H. Pepys, Esq. on respiration, 404
Ammonia, an account of its analysis, by combustion with oxygen and other gases, - - - - 430
——— experiments to ascertain if water is formed during its electrization, - - - - 432
——— of its expansion by electrization, - - 434
——— of the proportions of hydrogen and nitrogen in it, 435
——— further inquiries on the action of potassium on it; and on its analysis, - - - - 430
Amnios, liquor of the, on its composition, - - 393
Apparatus for dividing instruments described, - 116
Arseniat of lead, native, description and analysis of it, 195
Arteries, functions of, - - - - - 1
Azote, experiments to ascertain if it is evolved when oxygen is respired, - - - - 413

B

Basis, boracic, experiments on, - - - 78
Battery, Voltaic, Mr. Children's, experiments on the best form of its construction, - - - - 38
Beam-compass, its application to the division of instruments, 221
Bile, on its composition, - - - - 382

page

Bird. His method of dividing instruments noticed, - 109
Bisection, method of performing described, - - 222
Blood, experiments on, by Voltaic electricity, - 385
Boracic acid, its decomposition and recomposition, - 76
Bow, the Newtonian prismatic blue, particulars relating to, 266
—— prismatic red, an account of, - - - 268
Bows, of a sudden change of their colours, - - 272
—— explanation of various appearances relating to them, 276
Bow-streaks, the cause of their colours, - - 292
Brande, Mr. William. A chemical analysis of the fluid contained in the intervertebral cavity of the squalus maximus, - - - - - 134
—— Observations on albumen and some other animal fluids, with remarks on their analysis by electrochemical decomposition, - - - 373
Britannia East India ship, her loss referred to, - 400
Brodie, Mr. B.C. Account of the dissection of a human fœtus, in which the circulation of the blood was carried on without a heart, - - - - - 161

C

Canal in the medulla spinalis described, - - 146
Carbon and hydrogen, experiments on the electrization of their aeriform combinations, - - - - 446
Carbonaceous matter, further inquiries respecting it, - 466
Cavendish, Henry, Esq. On an improvement in the manner of dividing instruments, - - - 221
Charcoal, its nature, - - - - 72
Children, John George, Esq. An account of some experiments on the best form for the construction of a Voltaic battery, - - - - - 32
Circles, astronomical, examination of the divisions upon, - 235
Circle, French, of, repetition referred to, - 242
Cline, Mr. Observations on his opinion concerning the propagation of animals, - - - - 359
Columbite, its analysis, - - - - - 243

D

Davy, Mr. His theory of the Voltaic apparatus noticed, 34
Davy, Humphry, Esq. His Bakerian lecture on some new analytical researches on the nature of certain bodies, particularly the alkalies, phosphorus, sulphur, carbonaceous matter, and the acids hitherto undecomposed, - 39

page

DAVY, HUMPHRY, Esq. New analytical researches on the nature of certain bodies, being an appendix to the Bakerian lecture for 1808, - - - - 450
Diamond, its nature, - - - - - 73

E

EARLE, Sir JAMES. An account of a calculus from the human bladder of uncommon magnitude, - - - 303
Ellipsoids, homogeneous, of the different methods pursued by geometers to ascertain their attractions, - - - - 345
Expectorated matter, on the sensible properties of that secreted by the bronchial membrane, - - - - - 313
——— how affected by different agents, - - 322
——— on the composition of different kinds, 541
——— on the distinction between it and other secreted animal fluids, - - - - - 345

F

Fluid, a chemical analysis of that contained in the intervertebral cavity of the squalus maximus, - - - 184
Fluoric acid, on its decomposition, - - - - 95
Fœtus, human, an account of one, - - - - 161

G

GAY LUSSAC and THENARD, their experiments on potassium controverted, - - - - - 42
GERSTNER, his experiments on the passage of water through tubes mentioned, - - - - - 7
Goniometer, reflective, its description, - - - - 253
GREGOR, Rev. WILLIAM. On a native arseniate of lead, 195

H

HENRY, WILLIAM, M. D. Experiments on ammonia, and an account of analyzing it, by combustion with oxygene and other gases, - - - - - - 490
HERSCHEL, WILLIAM, LL. D. Continuation of experiments for investigating the cause of coloured concentric rings, and other appearances of a similar nature, - - 259
HINDLEY. His method of dividing instruments noticed, - 112
HOME, EVERARD, Esq. On the nature of the intervertebral substance in fish and quadrupeds, - - - 177
——— Hints on the subject of animal secretions, - - - - - - - - 385

INDEX.

page

Home, Everard, Esq. Anatomical account of the equalis maximus, - - - - - 212

Hydrogen and Oxygen, experiments on the respiration of a mixture of these gases, - - - - 441

I

Instruments astronomical, an account of a method of dividing them and other instruments by ocular inspection, - 105

——— on an improved method of dividing them, - - - - - - - 221

——— on a method of examining their divisions, - - - - - - 232

Ivory, James. On the attractions of homogeneous ellipsoids, 345

K

Knight, T. A. Esq. On the origin and formation of roots, 169

——— on the comparative influence of male and female parents on their offspring, - - - 392

L

Lax, Rev. Wm. on a method of examining the divisions of astronomical instruments, - - - - 232

Lecture, the Bakerian, on some new analytical researches on the nature of certain bodies, - - - - 39

Lens, what considered, - - - - - 294

Libavius, liquor of, experiments on, - - - - 93

Linnæus. His opinion that the character of the male parent predominated in the exterior parts both of plants and animals controverted, - - - - 392

M

Milk, on its composition, - - - - - 312

Mucus, observations on - - - - - 373

——— *of the oister*, on its composition - - - 351

——— *of the trachea*, on its composition, - - - 312

Muriatic acid, experiments on, - - - - 91

——— ——— further enquiries respecting, - - - 463

N

Newtonian alternate fits of easy reflexion and transmission, remarks on - - - - - - 302

O

Oxygen gas, experiments on its respiration, - - - 415

page

P

PEARSON, GEORGE, M. D. On expectorated matter, - 313
Phosphorus, decomposition of, - - - - 67
Plumbago, experiments on, - - - - 70
Potassium, its action on ammonia, - - - - 40
——— of its solution in hydrogen, - - - - 57
——— of its alloy with sodium, - - - - 59
——— of its action on boracic acid, - - - 77
——— on fluoric acid, - - - 86
——— on muriatic acid, - - - 91
——— on sulphuretted hydrogen, - - 63
Presents received by the Royal Society from November 1808 to June 1809, - - - - 371
Prism, how concerned in the formation of the blue bow and streaks, - - - - - - 230
Pus, on its composition, - - - - 314
Pyrophorus, *Homberg's*, probably formed by the decomposition of potash, - - - - 100

Q

Quinquesection, methods of performing it described, - 225

R

Ramsden. His method of dividing noticed, - - 112
RENNELL, JAMES, ESQ. on the effects of westerly winds in raising the level of the British channel - - - 400
Respiration, on - - - - - 404
Rings, *concentric*, continuation of experiments for investigating the cause of them, and of other appearances of a similar nature, - - - - - 259
——— coloured, their production, - - 264
——— Newtonian coloured, on the primary cause of their origin, - - - - - 299
Roots, on their origin and formation, - - - 169
——— their functions, - - - - 175

S

Saliva, on its composition, - - - - 380
Secretions, *animal*, hints on the subject, - - 385
——— their probable cause, - - - *ib.*
Serum of the blood, action of voltaic electricity on it, - 389
SEWELL, MR. WM. A letter on a canal in the medulla spinalis of some quadrupeds, - - - - 146

INDEX.

	page
Sex, remarks on it in domesticated animals,	397
Skate, on its spine,	179
Smeaton, his method of dividing noticed,	118
Squalus maximus, observations on its spine,	177
——— an anatomical account of one,	212
——— some observations on one,	213
——— its identity with the animal imagined to be a sea snake attempted to be proved,	215
Sturgeon, on its spine,	180
Sulphur, its decomposition,	59
——— *and phosphorus*, further inquiries concerning them,	462

T

Table, a numerical one, of elective attractions, with remarks on the sequences of double decompositions,	141
Tantalite, its analysis,	246
TROUGHTON, Mr. EDWARD. An account of dividing astronomical and other instruments by ocular inspection; in which the usual tools for graduating are not employed; the whole operation being so contrived, that no error can occur but what is chargeable to vision, when assisted by the best optical means of viewing and measuring minute quantities,	105

W

Westerly winds, on their effects in raising the level of the British channel,	400
WOLLASTON, WM. HYDE, M. D. On platina and native palladium from Brasil,	189
——— On the identity of columbium and tantalum,	246
——— description of a reflective goniometer,	253

Y

YOUNG, THOMAS, M. D. A numerical table of elective attractions; with remarks on the sequences of double decompositions,	141

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